THE

AMERICAN JOURNAL

OF THE MEDICAL SCIENCES

FOR OCTOBER 1854.

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TO READERS AND CORRESPONDENTS.

The following works have been received:—

Anatomical and Physiological Observations. By JOHN STRUTHERS, M. D., Fellow of the Royal College of Surgeons of Edinburgh, Lecturer on Anatomy. Part I. Edinburgh: Sutherland & Knox, George Street; Simpkin, Marshall, & Co., London, 1854. (From the Author.)

A Short Exposition of the Circulation and Nervous System, with Reference to Disease and Treatment. By G. HAMILTON BELL, F. R. C. S. E., formerly of the Madras Medical Establishment. Edinburgh. Maclachlan & Stewart. London: John Churchill, 1854. (From the Author.)

Case of Strangulation of the Jejunum released by Gastrotomy. With Observations on the Diagnosis and Treatment of Intestinal Obstructions within the Abdomen. By JOSEPH RIDGE, M. D. Read before the Hunterian Society, June 18, 1854. Reprinted from the Association Medical Journal. London, 1854. (From the Author.)

The Modern Treatment of Syphilitic Diseases, both Primary and Secondary, Comprising the Treatment of Constitutional and Confirmed Syphilis by a safe and successful Method. With numerous Cases, Formulæ, and Clinical Observations. By LANGSTON PARKER, Surgeon to Queen's Hospital, Birmingham. From the third and entirely rewritten London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

A Clinical Introduction to the Practice of Ausculation and other means of Physical Diagnosis, in Diseases of the Lungs and Heart. By H. M. Hughes, M. D., Fellow of the Royal College of Physicians, Assistant Physician to Guy's Hospital, etc. Second American, from the second and revised London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

Healthy Skin; a Popular Treatise on the Skin and Hair—their Preservation and Management. By ERASMUS WILSON, F. R. S., Author of "A Treatise on Diseases of the Skin," etc. etc. Second American from the fourth and revised London edition. With illustrations. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

The Principal Forms of the Skeleton and of the Teeth. By Professor R. OWEN, F. R. S. &c., Author of "Odontography," "Lectures on Comparative Anatomy," "Archetype of the Skeleton," "On the Nature of Limbs," etc. etc. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

The Principles of Animal and Vegetable Physiology: A Popular Treatise on the Functions and Phenomena of Organic Life. To which is prefixed a general view of the great Departments of Human Knowledge. By J. STEVENSON BUSHNAN, M. D., Physician to the Metropolitan Free Hospital, etc. etc. With one hundred and two illustrations on wood. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases, frequently mistaken for, or associated with Phthisis. By JOHN HUGHES BENNETT, M. D., F. R. S. E., Prof. of the Institutes of Medicine and Clinical Medicine in the University of Edinburgh, etc. etc. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

Principles of Comparative Physiology. By WILLIAM B. CARPENTER, M. D., F. R. S., F. G. S., Examiner in Physiology and Comparative Anatomy in the University of London; President of the Microscopical Society of London, etc. etc. With three hundred and nine wood engravings. A new American, from the fourth and revised London edition. Philadelphia: Blanchard & Lea, 1854. (From the Publishers.)

An Examination of the Practice of Bloodletting in Mental Disorders. By PLINY EARLE, M. D., Author of "A Visit to Thirteen Asylums for the Insane in Europe;" "History, Description, and Statistics of the Bloomingdale Asylum for the Insane," etc. New York: S. S. & W. Wood, 1854. (From the Publishers.)

Institutions for the Insane in Prussia, Austria, and Germany. By PLINY EARLE, M. D., one of the Visiting Physicians to the Lunatic Asylum of the City of New York; late Physician to the Bloomingdale Asylum, etc. New York: S. S. & W. Wood, 1854. (From the Publishers.)

The Dispensatory of the United States of America. By GEORGE B. WOOD, M. D., Professor of the Theory and Practice of Medicine in the University of Pennsylvania; President of the College of Physicians of Philadelphia; one of the Physicians to the Pennsylvania Hospital, etc. etc., and FRANKLIN BACHE, M. D., Professor of Chemistry in the Jefferson Medical College of Philadelphia; President of the American Philosophical Society, etc. etc. Tenth edition, carefully revised. Philadelphia: Lippincott, Grambo, & Co., 1854. (From the Authors.)

Auscultation and Percussion. By Dr. JOSEPH SKODA. Translated from the fourth edition, by W. O. MARKHAM, M. D., Assistant Physician to St. Mary's Hospital. Philadelphia: Lindsay & Blakiston, 1854. (From the Publishers.)

Materia Medica, or Pharmacology and Therapeutics. By WILLIAM TULLY, M. D. Vol. I. No. 12. October, 1853. Springfield, Mass.: Published by Jefferson Church, M. D., 1853.

Manual of Human Microscopical Anatomy. By A. HOLLIKER, Professor of Anatomy and Physiology in Würzburg. Translated by GEORGE BUSK, F. R. S., and THOMAS HUXLEY, F. R. S. Edited, with notes and additions, by J. DA COSTA, M. D. Illustrated by three hundred and thirteen engravings on wood. Philadelphia: Lippincott, Grambo, & Co., 1854. (From the Publishers.)

Observations on the Cause, Nature, and Treatment of Epidemic Cholera. By A. B. PALMER, M. D., Professor of Anatomy in the University of Michigan. Detroit, 1854.

Paper read before the Medical Society of the State of New York, on, first, Penetrating Wounds of the Abdomen with Punctured Wounds of the Intestines; second, Penetrating Wounds of the Larynx, and their Treatment, with Cases for Illustration. By ALDEN MARCH, M. D., Professor of Surgery in Albany Medical College. Albany, 1854.

On the Reputed Causes of Yellow Fever, and the so-called Sanitary Measures of the Day. By M. MORTON DOWLER, M. D. Fourth District, New Orleans. New Orleans, 1854.

Statistics of Consumption in Roxbury. Read before the Norfolk District Medical Society of Massachusetts, at the Annual Meeting, May 17, 1854. (Printed by vote of the Society.) By B. E. COTTING, M. D. With an Appendix. Boston, 1854.

Observations on Exhaustion from the Effects of Heat. (*Coup de Soleil.*) By H. S. SWIFT, M. D., Resident Physician of the New York Hospital. (From the New York Journal of Medicine.) New York, 1854.

Observations on the Asiatic Cholera, as it appeared in Cincinnati in 1849–50. By THOS. CARROLL, M. D. Reprinted from the Western Lancet for June, 1854.

Remarks on the Medicinal Properties of Bailey's Springs, Lauderdale County, Ala., and Cooper's Well, Hinds County, Miss. By S. C. FARRAR, M. D., of Jackson, Miss. Jackson, 1854.

Poisoned Wounds; their Distinctive Features, Classification, with Remarks upon the Classes and a Special Treatise upon the Nature and Treatment of Wounds resulting from the Bites of Venomous Reptiles, Experiments, &c., being a Report of a Committee to the Medical Association of Missouri. By A. F. JETER, M. D., of Palmyra, Mo., Chairman of Com. Quincy, 1854.

Sixth Annual Report of the Massachusetts School for Idiotic and Feeble-minded Youth. Boston, 1854.

Report of the Sanitary Committee of the Board of Health upon the subject of Slaughter-houses and Soap-boiling establishments in cities—their effects upon Public Health, together with the location of Cow-stables in cities, and the effect of feeding Still-Slops upon the Milk, &c. Made to the Board, Aug. 22, 1854, and unanimously adopted and ordered to be published in pamphlet form for gratuitous distribution. Philadelphia, 1854.

The Transactions of the Iowa State Medical and Chirurgical Society. Third and fourth Sessions, held at Fairfield, May, 1852, and Davenport, June, 1853. Burlington, 1854.

Transactions of the Medical Association of Southern Central New York, at the Eighth Annual Meeting, held at Homer, June, 1854. Auburn, 1854.

Transactions of the Fifth Annual Meeting of the Medical Society of the State of North Carolina, held at Raleigh, N. C., May, 1854. Wilmington, N. C., 1854.

Annual Report of the City Inspector of the City of New York, for the year 1853. New York, 1854.

Cataract and Amaurosis.—Clinical Lectures on some of the Principal Diseases of the Eye, delivered at the New York Medical College, in May, June, and July, 1854. By Isidor Gluck, M. D. New York, 1854. (From the Author.)

These interesting lectures reached us too late for notice in our present No.; they shall receive attention in our next.

The Physician's Visiting List, Diary, and Book of Engagements for 1855. Lindsay & Blakiston. (From the Publishers.)

We take pleasure in announcing the early appearance of this most useful—we had almost said indispensable—little volume.

The following Journals have been received in exchange:—

Annales Médico-Psychologiques, Redige par MM. Baillarger, Brierre de Boismont et Cerise. July, 1854.

Revue de Thérapeutique Médico-Chirurgicale. Par A. Martin-Lauzer. June, July, Aug. 1854.

Gazette Médicale de Paris. May, June, July, 1854.

Le Moniteur des Hôpitaux. Rédacteur en Chef, M. H. De Castelnau. June, July, Aug. 1854.

Medical Times and Gazette. June, July, Aug., 1854.

Dublin Medical Press. June, July, Aug., 1854.

The Retrospect of Medicine. Edited by W. Braithwaite, M. D. January to June, 1854.

The Half Yearly Abstract of the Medical Sciences. Edited by W. H. Ranking, M. D., and C. B. Radcliffe, M. D. Jan. to June, 1854.

The British and Foreign Medico-Chirurgical Review. July, 1854.

The Indian Annals of Medical Science; a half-yearly Journal of Practical Medicine and Surgery. April, 1854.

The Journal of Psychological Medicine and Mental Pathology. Edited by Forbes Winslow, M. D. July, 1854.

Monthly Journal of Medical Science. June, July, 1854.

Association Medical Journal. Edited by John Rose Cormack, M. D. June, July, 1854.

The Glasgow Medical Journal. April, July, 1854.

The Medical Chronicle, or Montreal Monthly Journal of Medicine and Surgery. Edited by William Wright, M. D., and D. C. McCallum, M. D. July, Aug., Sept., 1854.

The New York Journal of Pharmacy. Edited by Thomas Antisell, M. D., aided by Prof. Torrey, Dr. Enderlin, and B. Canavan. July, August, September, 1854.

The Medical Reporter. Published under the direction of the Chester and Delaware County Medical Societies. July, 1854.

Iowa Medical Journal. Conducted by the Faculty of the Medical Department of the Iowa University. June, 1854.

Indiana Medical Journal. Edited by Drs. Byford and Reynolds. June, 1854.

American Journal of Pharmacy. Edited by William Procter, Jr. July, September, 1854.

The American Journal of Science and Arts. Conducted by Profs. B. Silliman, B. Silliman, Jr., and James D. Dana, in connection with Profs. Gray and Agassiz, and Drs. Burnett and Gibbs. July and Sept. 1854.

The New York Journal of Medicine. Edited by S. S. Purple, M. D., and S. S. Smith, M. D. July, September, 1854.

The Medical Examiner. Edited by Samuel L. Hollingsworth, M. D. July, August, September, 1854.

The New York Medical Gazette. Edited by D. Meredith Reese, M. D. July, August, September, 1854.

The North-Western Medical and Surgical Journal. Edited by W. B. Herrick, M. D., and H. A. Johnson, M. D. May, July, 1854.

Southern Med. and Surg. Journal. Edited by L. A. Dugas, M. D. July, 1854.

The Southern Journal of the Medical and Physical Sciences. Edited by Drs. King, Jones, Curry, and Wood. July, 1854.

New York Medical Times. Edited by H. D. Bulkley, M. D. July, August, September, 1854.

The American Medical Monthly. Edited by Edward H. Parker, M. D. July, August, September, 1854.

The New Orleans Medical and Surgical Journal. Edited by Bennet Dowler, M. D. July, September, 1854.

The Stethoscope. Edited by Drs. Atkinson, Haxall, Bolton Lewis, etc. July, August, September, 1854.

The Ohio Medical and Surgical Journal. Edited by John Dawson, M. D. July, September, 1854.

The New Hampshire Journal of Medicine. Edited by G. L. Hubbard, M. D. July, August, September, 1854.

New Orleans Medical News and Hospital Gazette. Edited by Drs. Choppin, Beard, Schlater, and Boyer. July, September, 1854.

The American Journal of Insanity. Edited by the Officers of the New York State Lunatic Asylum. July, 1854.

The Boston Medical and Surgical Journal. Edited by J. V. C. Smith, M. D., and Geo. S. Jones, M. D. July, August, September, 1854.

Buffalo Medical Journal. Edited by Austin Flint, M. D., and S. B. Hunt, M. D., July, Aug., Sept., 1854.

The Western Journal of Medicine and Surgery. Edited by Lunsford P. Yandell, M. D. June, July, September, 1854.

Kentucky Medical Recorder. Edited by H. M. Bullitt, M. D., and John Bartlett, M. D. June, 1854.

Charleston Medical Journal and Review. Edited by D. J. Cain, M. D., and F. Peyre Porcher, M. D. July, September, 1854.

The Western Lancet. Edited by L. M. Lawson, M. D., and T. Wood, M. D. July, August, September, 1854.

The Peninsular Journal of Medicine. Edited by A. B. Palmer, M. D., and E. Andrews, M. D. July, August, 1854.

The New Jersey Medical Reporter and Transactions of the New Jersey Medical Society. S. W. Butler, M. D., Editor; Jos. Parrish, M. D., Associate Editor. August, September, 1854.

St. Louis Medical and Surgical Journal. Edited by M. L. Linton, M. D., and W. M. McPheeters, M. D. July, 1854.

Memphis Medical Recorder. Edited by A. P. Merrill, M. D., and C. T. Quintard, M. D. July, 1854.

Nashville Journal of Medicine and Surgery. Edited by W. R. Bowling, M. D., and Paul F. Eve, M. D. March, April, June, July, August, 1854.

The American Journal of Dental Science. Edited by Dr. C. A. Harris, A. A. Blandy, and A. S. Peggot. July, 1854.

The Virginia Medical and Surgical Journal. Edited by Drs. J. B. McCan, J. F. Peebles, and G. A. Otis. No. XVI.

Communications intended for publication, and Books for Review, should be sent, *free of expense*, directed to Isaac Hays, M. D., Editor of the American Journal of the Medical Sciences, care of Messrs. Blanchard & Lea, Philadelphia. Parcels directed as above, and (carriage paid) under cover, to John Miller, Henrietta Street, Covent Garden, *London;* or M. Hector Bossange, Lib. quai Voltaire, No. 11, *Paris*, will reach us safely and without delay. We particularly request the attention of our foreign correspondents to the above, as we are often subjected to unnecessary expense for postage and carriage.

All remittances of money, and letters on the *business* of the Journal, should be addressed *exclusively* to the publishers, Messrs. Blanchard & Lea.

☞ The advertisement-sheet belongs to the business department of the Journal, and all communications for it should be made to the publishers.

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BIBLIOGRAPHICAL NOTICES.

XV. Transactions of the Medical Association of the State of Alabama, at its Seventh Annual Session, begun and held in the City of Montgomery, Jan. 10–12, 1854. 8vo. pp. 190.

Transactions of the Medical Society of the State of Pennsylvania, at its Annual Session, held in the City of Pottsville, May, 1854. Vol. IV. Published by the Society. 8vo. pp. 171.

Transactions of the Medical Association of Southern Central New York, at the Eighth Annual Meeting, held at Homer, June, 1854. 8vo. pp. 120.

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XVII. On the Structure and Use of the Spleen. By Henry Gray, F. R. S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum of St. George's Hospital. London, 1854: 8vo. pp. 380. - - 491

XVIII. Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm—their Pathology and Treatment. By Charles Bland Radcliffe, M. D., Licentiate of the Royal College of Physicians, Assistant Physician to the Westminster Hospital, Lecturer on Materia Medica and Therapeutics at the Westminster Hospital School of Medicine, etc. etc. London, 1854: 8vo. pp. 144. - - 492

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ERRATA.

Page 331, line 15, *for* 7,688 *read* 7,888.
" " " 19, " facts *read* fats.
" 334, " 33, " arrests *read* arrest.
" 336, " 24, " besides which *read* besides those which.

Fig. 1.

Fig. 2.

Parts concerned in a Popliteal Aneurism after cure by compression.

THE

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OF THE MEDICAL SCIENCES

FOR OCTOBER 1854.

ART. I.—*A Statement of the Appearance of the Parts concerned in a Popliteal Aneurism, which had been cured by Compression.* By J. KNIGHT, M. D., Professor of the Principles and Practice of Surgery, in the Medical Institution of Yale College. [With a plate.]

IN the number of this Journal for July, 1848, taken from the *Boston Med. and Surg. Journal*, of May 10, 1848, will be found the case of H. Johnson, in which a popliteal aneurism was successfully treated by pressure. The case was one of the ordinary kind of popliteal aneurism, the peculiarity of it being the manner in which compression on the artery was made by the fingers of assistants instead of instruments, many of which were tried and discontinued, on account of the severe pain produced by them; and the speedy and easy cure which was effected, the patient scarcely suffering from pain, and the treatment completed in about forty hours.

A statement of the progress of this case, together with the appearance of the parts after death, is of sufficient interest to be recorded. I am not aware that there is any published account of the state of the arteries and other parts concerned in an aneurism, after a cure by compression. If there is any such, it has escaped my notice. I regret that in this case a more perfect examination could not be made; and especially that the vessels could not have been injected before they were removed, so that the number and course of the anastomosing arteries could have been particularly described. This, however, could not be done, and the only means allowed us, was to remove the parts from the popliteal space, and to examine them afterwards, at our own leisure. This was done with a good deal of care, and the accompanying drawing is a correct representation of the arteries and neighbouring parts.

In the statement of the case, it is mentioned that after the cure was effected,

the femoral artery could be felt pulsating naturally down to the point of its passage through the tendon of the adductor longus, and not beyond this, and that large subcutaneous arteries were noticed upon the inside of the knee. At a little later period, the anterior and posterior tibial arteries, which could not be felt for a time, were distinguished pulsating distinctly, but not very strongly. This state of the arteries continued during life. About six months after the cure of the aneurism, he injured the leg by a blow, and there resulted an ulcer over the tibia, which was at times exceedingly troublesome, and continued until his confinement by his last sickness, when it healed. The disease of which he died was a schirrous tumour, involving the anterior portion of the stomach, from the pylous to the great extremity. He suffered from this about six months before his death, and died exhausted by great pain, vomiting, and inanition. He died about seven years and a half after the operation.

The parts from which the drawing was made, were prepared with much care. The vessels, especially the arteries, were repeatedly inflated under water, that every branch might be discovered and tied, when they were injected with coloured wax; so that I am confident that no arterial branch escaped detection. The disease was on the left leg, and the view given in the drawing is the anterior face of the parts, that which looked towards the bones; the artery being the most deeply seated, the vein upon the right, and the nerve still further to the right, and more superficial, as measured from the surface of the limb. In the drawing, as they are viewed from before, they appear in a position the reverse of this.

It will be seen that the femoral artery is of its full size, quite down to the place of the tumour. From this point up to its passage into the abdomen, either where the pressure was made, or elsewhere, there was nothing unnatural in its appearance. Below the tumour, there is a portion of the artery, about two inches in length, before the first branch at E is given off; there the artery was somewhat smaller than natural, and its coats were slightly thickened. It is commonly and truly in most cases stated, that an obstructed artery becomes impervious to the place of the origin of its first branch. This is true in the present case of the artery above the tumour, while below there is a portion of it two inches in length, with its canal fully open, and of nearly the natural size, before the origin of its first branch. As this was a point to which our attention was especially directed, there can, I think, have been no mistake about it.

The remains of the aneurismal tumour, seen at H, not only occupied the space between the extremities of the obliterated artery, but extended three-fourths of an inch or more upon the arteries above and below, and was closely adherent to the accompanying vein. When freed from the surrounding cellular tissue, to which it adhered firmly, it was about three-fourths of an inch thick, of a tawny hue, slightly tinged with red, and of a dense, firm, almost fibrous texture. I could discover in it none of the peculiar arterial tissue.

DESCRIPTION OF THE PLATE.

Fig. I.—A. Femoral artery, above the tumour.

B. Artery below the tumour. This is shown a little larger than it is in the preparation.

C. Large anastomosing branch passing around upon the inside of the knee.

D. Smaller branches.

E. First branch given off below the tumour.

F. Femoral vein, of the natural size, with many branches, which are not shown.

G. Popliteal nerve.

H. Condensed membrane of a firm fibrous texture, filling up the space between the extremities of the artery, and involving the vein. This was about three-fourths of an inch thick, and was all that remained of the aneurismal tumour. There was nothing of it between the ends of the artery that could be distinguished as arterial texture.

Fig. II.—This shows at A, the size and form of the inside of the artery, the injected material being laid bare by an incision through the coats of the artery.

Art. II.—*On Digestion of Fatty Matters by the Pancreatic Juice.* By S. Jackson, M. D., Professor of Inst. Med. in the University of Pennsylvania.

The different digestions or chemical processes to which the different kinds of food—albuminous, oleaginous, saccharine, or amylaceous—undergo in the alimentary canal, have been the subject of repeated investigation and experiment since Spallanzani first demonstrated them to belong to the domain of chemico-physical science.

Prout determined the differences in the nature of the alimentary substances. His observations were followed by the more accurate researches of Mulder, Dumas, Becquerel, Liebig, and the pupils of the Geissen school. They investigated the chemical elementary composition, the modes of production, decomposition, metamorphoses, and other relations of those substances. The way was thus opened for acquiring a more correct knowledge of the kind of chemical process, or digestion, appropriate to each kind of aliment, and the seat or organ in which it occurred.

Approximations are being made to the solution of these interesting problems, which are not devoid of practical importance. They still remain unsettled and obscured from the conflicting statements, as to facts, of different observers, entitled equally to confidence from their intelligence, skill, and truthfulness.

It is not my intention to enter on the general question of the alimentary digestions. I shall confine myself to one alone, that of the oily or fatty matters, respecting which there exist opposing statements as to the observation of a simple phenomenon, which is, however, the principal fact of the process.

MM. Cl. Bernard and Bousingault, and Bouchardat and Sandras, had shown that these substances were converted into an emulsive fluid. This emulsion, absorbed by the lymphatics of the villi (to which the term lacteals, in my opinion, alone is properly applicable), constitutes the milk-like fluid, named chyle, found in the lymphatics of the mesentery after a meal containing oleaginous principles; and at no other time. If the term chyle is restricted to this milk-like fluid, which gave origin to it, then chyle is not the general product of digestion, as it is generally supposed to be, but is the fatty matters of the food in a state of emulsion, and nothing more.

Subsequently, M. Cl. Bernard demonstrated that this emulsion is produced by the action of the pancreatic juice: and he assumed it to be the essential agent in its formation. This demonstration appeared conclusive, from one of the most neat and beautiful observations in physiology.

It is known that in the rabbit, the insertions of the hepatic and pancreatic ducts in the duodenum, are separated by an interval of near six inches. M. Bernard availed himself of this fact to observe, in nature—in situ—the seat and act of the conversion of fats into an emulsion. He accordingly introduced into the stomach of rabbits, after fasting, some oil along with green food, and after the lapse of a period of time, not related, the animals were opened, and the intestines exposed. No chyle or emulsion of fats was found in the mesenteric lymphatics at and below the opening of the hepatic duct, while they were distended with the chylous or fatty emulsion at and below the insertion of the pancreatic duct. Further, when the pancreatic juice is added to sweet-oil or other fluid fat, an emulsion is instantly produced; and this emulsion is very perfect and permanent. The conclusion from this experiment was, that the emulsion of the oil was the immediate result of the action of the pancreatic juice.

The correctness of this observation of M. Bernard, has been denied by MM. Bidder, Schmidt, Lehmann, Donders, and Herbert. It is asserted that M. Bernard was misled by not properly timing his observations. It is stated, that if the animal be examined one or two hours after the ingestion of oil into the stomach, the lymphatics between the biliary and pancreatic ducts are then tinged with white fatty emulsion, which, consequently, is formed independent of the pancreatic fluid. When the examination is delayed for five or six hours after the introduction of oil into the stomach, it is alleged that the emulsion formed in the upper portion of the duodenum has then descended, and the portion absorbed has disappeared from the lymphatics, having passed on to the thoracic duct. By this time, the emulsion having reached the region of the pancreatic duct, and passed into the jejunum, the lymphatics of the mesentery of this part of the intestines are necessarily turgid with emulsified fat. It is

inferred, therefore, that M. Bernard did not examine the animals on which he experimented under the period of five or six hours after giving the oil, and was thus led into a misrepresentation of the true character of the phenomena.

This simple fact, it would seem, ought to be settled without difficulty; but the reports of those who have observed the phenomena are entirely adverse to each other. My engagements in the practice of my profession interfere with experimental researches; they prevent the application of time requisite to success in experiments. Dr. James Corse obligingly offered to perform the experiments for me, submitting to my observations the phenomena on the opening of the animals.

The following are the results of this investigation:—

A number of rabbits were procured. They had been fed for some days on hay. Four of them were deprived of food for three days. Three drachms of fresh sweet-oil were then introduced into the stomach of each animal, through a tube, successively, at nine, ten, eleven, and twelve o'clock, and immediately fed with some peas. At 2 P.M., varying from five to two hours after giving the oil, they were made insensible with sulphuric ether, the abdomen opened, and the intestines and mesentery spread out.

In one only had any emulsion of the oil been formed. In that one the mesenteric lymphatics, commencing half an inch below the pancreatic duct, were turgid with white emulsive fluid. Not a single lymphatic could be seen in the transparent mesentery between the pancreatic and hepatic ducts.

On examining the stomachs, they were found distended, in each rabbit, with a green, comminuted mass of vegetable matter, which must have proceeded from the hay on which they had been previously fed, and had remained undigested. In one, a portion of the oil was free in the stomach. In all the others, most of it appeared to have been absorbed and retained by the large vegetable bolus filling up the stomach.

The above experiments indicate a source of fallacy in making this observation. The food on which the animal has fed may interfere with its passage through the stomach, and cause a longer retention of it there than is suspected.

Second Series of Experiments.—To obviate the cause of failure in the preceding experiments, the remainder of the rabbits were fed for eight days on green cabbage. It was found that they suffered, and two died, under this exclusive aliment. It was necessary to add wheat-bran, which was eaten with avidity, and soon restored them to a good condition.

Four more were selected and kept without food for two days. They were then fed with cabbage saturated with sweet-oil, at intervals, from 9 A.M. until 2 P.M. In this manner oleaginous matter was constantly entering the digestive apparatus for five hours, until the moment of observation. If oils are emulsified in the stomach or duodenum, independent of the pancreatic juice, the lymphatics above the connection of the pancreatic duct, would, of course, be found to contain the emulsion.

The animals were destroyed by the same process as in the foregoing experiments.

The first rabbit was feeding up to the time it was killed. The lymphatics between the hepatic and pancreatic ducts were invisible, except two, from four to six lines above the pancreatic duct. They appeared as faint white lines. Below the pancreatic duct they were distended with chyle.

The second rabbit, under the same circumstances, presented the same appearances. No chyle was contained in the lymphatics until within a few lines of the pancreatic duct, where two or three were faintly visible. Below the duct they were turgid with chyle.

The third, a well-grown rabbit, gave precisely the same results. There was no appearance of chyle in the lymphatics of the mesentery, except within a few lines of the pancreatic duct, while below they were well filled with the emulsive fluid.

The fourth, also a large, strong animal, presented corresponding appearances. No chyle in the lymphatics, from the commencement of the duodenum until within a few lines of the pancreatic duct; below it they were distended with milky emulsion.

The above experiments corroborate the observations of M. Cl. Bernard, as respects the phenomena in rabbits, and do not accord with the statements of the German physiologists, MM. Bidder and Schmidt, Lehmann, and Frerichs.

An experiment relied on by MM. Schmidt, Bidder, Lehmann, and Frerichs, in opposition to M. Cl. Bernard, is wholly inconclusive. After tying the pancreatic duct in one instance, and the small intestines below the pancreatic duct in another, milk, or milk with olive-oil, was injected into the intestines, "and the lacteals were filled with white chyle." This was to be expected. What is the necessary condition for the absorption by the lacteals of the villi of fatty substances? That they exist as an emulsion. Milk is a natural emulsion, holding a fatty substance (cream) in suspension, and consequently in the condition for absorption. Milk forms an emulsion with oils, and creates the same condition. Still more extraordinary is the experiment of Frerichs, quoted against M. Bernard by Lehmann, in which he injected an *emulsion of oil and albumen* into the ligated intestine, and its absorption was considered as a refutation of M. Cl. Bernard's doctrine.

The albumen of egg forms a most perfect and a persistent emulsion with oils. The pure pancreatic juice does the same. This I have witnessed, and can state that the oil does not "soon separate again on the surface," as Lehmann asserts. The pancreatic juice, as Lehmann shows, differs very slightly from pure albumen. No other intestinal fluid possesses this character so strongly, and no other is as well adapted to emulsify the fats of our aliment.

The cystic bile is viscid from containing a larger amount of mucus derived from its mucous membrane, and by agitation forms an emulsion with oils. This emulsion has always appeared to me very inferior to that obtained from pure pancreatic juice, or albumen of egg. Neither is it as persistent, the oil

beginning in a few hours to separate. The hepatic bile is much thinner and contains much less mucus. It is unknown whether it will form an emulsion with fats. It may be considered as very doubtful. No positive facts are known as to the quantity of cystic bile that enters into the duodenum during digestion. The probability is, that it is very small in comparison to the amount of freshly secreted hepatic bile coming directly from the liver. This view gives additional confirmation to M. Bernard's doctrine.

The liquid albuminose resulting from the digestion of the albuminous principle of the food, which is found often in the intestinal canal during digestion, may, from its accidental presence, concur occasionally in the formation of the emulsion of oils. The pancreatic juice is, in the present state of our knowledge of the facts, the only intestinal humour that can exercise this office as a permanent function.

I recall a circumstance, now some twenty years past, an interesting fact in this discussion, which, at the time, was inexplicable. I assisted Dr. John Webster, late Professor of Anatomy in Geneva Medical College, in the autopsy of one of his patients, who had died after long suffering, and in a state of extreme marasmus, from severe and intractable dyspeptic symptoms. Cream had been the principal nutriment for several weeks, as rarely any other food would be retained. The disease proved to be a scirrhus of the pancreas. The colon was filled with a large quantity, some two or three pounds, of a yellow fatty substance, resembling a mixture of yellow ochre and butter. A large proportion of the fatty bodies of the cream had undergone no emulsive action from the absence of the pancreatic juice, and were not absorbed.

M. Cl. Bernard came to the conclusion that the fats were decomposed by the pancreatic juice. This is not probable. The emulsion of fats is a physical, not a chemical process. It consists of the mechanical division of fats into the minutest globules, each coated with a delicate film of the emulsifying body—albumen in chyle—casein in cream. If the experiment of Bouchardat and Sandras be correctly reported, it is conclusive on this point. They gave to a dog a considerable quantity of sweet almond-oil. After some hours the animal was killed. Some of the oil, unchanged, was found in the stomach and intestines. The lacteals were filled with white chyle, as also the thoracic duct. A portion obtained from this last was treated with sulphuric ether, and the almond-oil was obtained unchanged in its properties.

The principal fact relied on by the above German physiologist to controvert M. Bernard's theory of fatty decomposition by the pancreatic juice, is of no value. Butter was given to cats and dogs, and butyric acid was sought for, but not found in the intestines. This is considered as proof that the butter was not decomposed. It proves nothing of the kind. Butyric acid readily decomposes. It cannot resist the activity of the numerous chemical reactions at work in the alimentary canal during digestion. Lactic acid is being formed incessantly in the lungs and intestines from the metamorphosis of

glucose, sugar, and starch; it is as speedily decomposed. The same occurs with the bile. In the fecal matters, in a normal state of the bowels, very little of the biliary constituents, other than the colouring matter, is to be found. The conjugate biliary acids—glyco-cholic and tauro-cholic—disappear and are destroyed. Glycocol is not found; taurin, occasionally detected in the middle of the alimentary tract, disappears at the lower portion. The albuminous materials of the food that pass beyond the stomach, are metamorphosed in the intestines. In this conflict of molecular and chemical reactions, it would be impossible for butyric acid to exist. Its absence is no evidence that it was not eliminated.

The strongest objection to M. Bernard's doctrine is, that in an emulsion of oil formed with pure albumen the fatty body is not decomposed. An emulsion is a physical, not a chemical process.

It consists in the reduction of oil to the minutest particles—globulets—each covered with a coat of albumen when it is the emulsifying body. This is all that is required to impart to fats the capacity for absorption into, and their passage through tissues.

The adoption of M. Bernard's doctrine involves an inexplicable difficulty. If the neutral fats are decomposed by the pancreatic juice before, or at the time of absorption, as they are found to exist in the adipose tissue in their original state of neutral fats, they must be recomposed again in the blood, or at the instant of their secretion. To accomplish this change there must be a generation of oxide of lipyl and glycerin, the bases of neutral fats. It is difficult to understand how it can occur. It is true, Mulder has offered a very ingenious hypothesis to show the possibility of the formation of those bodies, in the economy, from lactic acid. But the whole matter is so entirely hypothetical, in its present state, that it does not comport with the more rigid canons of modern medical philosophy, and cannot be accepted.

The state of our knowledge may be summed up in the following conclusions:—

1. Liquid fats are not miscible with the aqueous albumino-saline fluid—liquor sanguinis—with which all the vascular tissues are saturated; it cannot enter their pores, and consequently cannot be absorbed.

2. Liquid fats, when emulsified by albumen, are reduced to minute particles, each coated with albumen. In this state they are miscible with the liquor sanguinis, moistening the tissues, can enter their pores, and are then capable of absorption. This is the sole condition requisite for the absorption of fats.

3. The white milk-like fluid, named chyle, is this emulsion of the fatty matters of the food mixed with the ordinary lymph always contained in the lymphatics of the alimentary canal, and other abdominal organs and mesentery. The molecular base of Gully is the microscopic appearance in the chyle of the minute globulets of fat coated with albumen.

4. Albumen forms a perfect and persistent emulsion with oils. The pancreatic fluid is a saturated albuminous solution, and forms with oils an emulsion equally as perfect and permanent as that of albumen.

5. The pancreatic juice is the only highly albuminous fluid in the alimentary canal, that can accomplish the formation of a perfect emulsion; and the opinion of M. Cl. Bernard, that this process is one of its functions, is, it appears to me, sustained.

6. The observations of M. Cl. Bernard, that the formation of the emulsion of fats in rabbits, is at and below the pancreatic duct, and not above it, is confirmed by the experiments reported in this communication. And, further, that the experiments on rabbits are the most reliable, as being a true exemplification of the natural process, unattended with violence and torture to the animals, more or less disturbing in their effects.

7. That M. Cl. Bernard's view of the decomposition of fats by the pancreatic juice is not proved, is opposed by the nature of the process, and by analogy with other emulsions: it is unnecessary to the accomplishment of the absorption of fats, and introduces other and complicated processes that are unknown to exist, and are mere hypotheses.

ART. III.—*On the Gastric Juice, and its Office in Digestion.* By JOHN C. DALTON, Jr., M. D., New York.

THOUGH the gastric juice has been more fully and perhaps more successfully studied than any other of the digestive fluids, there are still some contradictory statements with regard to it, even among authors of the first eminence. Since the admirable observations of Dr. Beaumont on the person of Alexis St. Martin, the secretion has been examined, on the lower animals, by Blondlot, Schwann, Wasmann, Bernard, Lehmann, and others, whose researches have, in most essential points, confirmed those of the American observer. During the past year, I have studied the gastric juice, at various times, by means of fistulæ, artificially established in the stomach of the dog, after the method of Blondlot and Bernard. The secretion was almost always obtained by feeding the animal with raw or cooked meat, after a twelve hours' fast, and collecting and filtering the fluid that ran from the fistula during the first half hour afterward. The statements of Dr. Beaumont with regard to the intermittent character of the secretion were always found to be correct. The stomach, in the intervals of digestion, contains only a colourless mucus, usually alkaline in reaction, though sometimes neutral. The acid fluid begins to run by drops in about five minutes after the introduction of food, and in ten to fifteen minutes it flows freely—occasionally in a small stream—from the orifice of the fistula. It then runs so fast that four or five ounces can be collected from a medium-sized dog in the course of half an hour. During the first five minutes of its flow it is sometimes colourless, but afterward it

has a faint amber tinge, resembling that of pale urine or the serum of the blood.

A little confusion of ideas seems to have existed in the minds of some observers with regard to the best method of obtaining gastric juice in a state of purity. Several have thought this could be best accomplished by irritating the gastric mucous membrane with substances not liable to be attacked by the secretion; as, for example, glass rods, metallic sounds, &c. Dr. Beaumont appears to have sometimes succeeded in obtaining a tolerably large quantity of fluid by this means. I have myself, however, never been able to obtain a sufficient quantity of the secretion in the dog by any other means than the introduction of food. The stomach will often refuse to secrete a single drop of acid liquor, though irritated almost constantly for a quarter of an hour by a metallic catheter. Other observers, also, subsequent to Dr. Beaumont, are all agreed that only a very small quantity can usually be collected by such means; and yet they continue to recommend and employ the method, under an idea that the gastric juice so obtained will be "purer" than that procured in any other way. A moment's consideration, however, will show that the fluid which is secreted under the influence of so artificial and unnatural a stimulus cannot be at all relied on as representing the real gastric juice; and that the only proper method of obtaining this is to present to the stomach its natural stimulus, viz. the food which its secretion is intended to digest. The mistake, however, has been frequently made, and will undoubtedly account for some incorrect statements with regard to the specific gravity of the gastric juice, its non-coagulability by heat, &c. Frerichs, for example, in *Wagner's Encyclopædia of Physiology* (Art. *Digestion*), directs that, in order to obtain a "pure" gastric juice, the animals in whom gastric fistulæ have been established "should be fed, while fasting, with *indigestible substances, pebble-stones, pepper-corns, &c.*, and the fluid from the stomach then examined." A singular attempt, certainly, to excite a "normal" secretion by means of substances which are altogether foreign and unnatural to it, and with which it has no physiological relation. The truth is, these authors make a pure assumption when they regard the fluid so collected as more "normal" than that obtained by other means. They do not seem, either to attach sufficient importance to the fact that these irritants can at best produce only a very scanty supply of the secretion. Frerichs himself says he has only been able to obtain in this way from forty-five drops to two drachms and a quarter of "pure" gastric juice. This is not surprising, since he employed the method least adapted to excite its secretion. The secretion of true gastric juice can, in fact, be excited only by food, and by those particular kinds of food which are to be digested by it. Loaf sugar, given to the dog, will be dissolved and disappear altogether from the stomach, as I have repeatedly found, without causing any appreciable amount of secretion. The same thing may be said of starch paste, when given alone. I have even seen tough and indigestible pieces of tendon, introduced through the fistula,

expelled again spontaneously, one after the other, without causing the flow of a single drop of acid fluid; while cooked meat, introduced in the same way, produced immediately a free secretion. It is evident, then, that to procure normal gastric juice, *i. e.* the fluid normally secreted during the process of digestion, we must make use of its natural stimulants; and we cannot hope to obtain it in any other way.

There are only two possible sources of impurity for the gastric juice taken from the stomach after the introduction of ordinary food. One is an admixture of saliva. This, however, is generally regarded as of little importance, and, at any rate, is not to be avoided by any of the methods proposed as substitutes. Saliva is even liable to be swallowed in the intervals of digestion, when no food whatever has been taken. The other source of impurity, and that which has been thought more important, is the danger that the fluid taken from the stomach, even at the commencement of digestion, may have already dissolved some of the elements of the food, and thus present to the experimenter a complicated solution, instead of pure gastric juice. This, however, is not the case. If the animal be fed upon firm pieces of fresh meat, containing little blood—particularly if the fluids have been previously coagulated by a short boiling—a sufficient length of time elapses before solution begins for the experimenter to obtain an abundant supply of the unaltered secretion. The two principal products of digestion, albumen and albuminose, may be readily detected if present; and experiment shows that they do not exist in the fluid obtained by this means. There is, then, really no objection to the employment of this method; which is, indeed, the only reasonable and physiological one.

Gastric juice, obtained as above, has the following properties: Its specific gravity is 1010; its acid reaction is always decided; its viscidity is so slight as to be hardly noticeable; it becomes turbid by boiling, and by an excess of alcohol; it is not troubled by nitric acid, nor by ferrocyanide of potassium.

Its *organic ingredient*, concerning which so much has been said, is undoubtedly the most important of its constituents. Though it has never been separated in a perfectly pure state, its presence is easily demonstrable. It may be the altered mucus of the stomach, as Bernard supposes, or a mixture of various organic matters, as believed by others; but it is certainly a peculiar substance, found nowhere else in the body, and essential to the digestive properties of the gastric juice. There seems no good reason why we should not retain for it the name of *Pepsin*, first given to it by Schwann; always recollecting, for the present, that this name does not indicate its chemical character or origin, but only its physiological destination. This pepsin resembles albumen, as stated by Robin and Verdeil, in being coagulated by heat. Lehmann and Frerichs are certainly in error when they assert the contrary; an error into which they have perhaps been led by regarding as true gastric juice an unnatural fluid, obtained by the unnatural means alluded to above. The coagulum thrown down on boiling fresh gastric juice is not

albumen, as Lehmann intimates, since it is not precipitated by either nitric acid or ferrocyanide of potassium; and after gastric juice has been boiled and filtered it has lost its digestive power, though its acid reaction remains unaltered.

With regard to the *mode of action* of gastric juice, it may be confidently stated that it acts only on the albuminoid substances, and articles of food containing them—as meat, eggs, bread, nutritious vegetables, &c. It is also true that these matters are mostly converted by the action of gastric juice into albuminose, *i. e.* a substance which is not coagulable by heat, or by nitric acid, but which is best precipitated by an excess of alcohol. If the different articles of food be digested in fresh gastric juice in test-tubes, at a temperature of 100° F., it will be found that the digestible ingredients of bread are all taken up under the form of albuminose. If cheese be treated in the same way, its casein is altogether converted into albuminose, and dissolved, while its oleaginous matters are set free, and collect as a yellow layer on the surface of the solution. A very considerable proportion of raw white of egg, however, dissolves in the gastric juice directly as albumen, and remains coagulable by heat. Soft-boiled white of egg and raw meat are principally converted into albuminose, but at the same time a small proportion of albumen is also taken up unchanged.

The gastric juice *actually dissolves the articles of food.* This has been, through some mistake, recently denied by one observer, so far as regards muscular tissue; the fibres being supposed to be simply swollen and partly disintegrated, but not actually dissolved in the stomach. The contrary, however, can readily be seen, by digesting raw meat, cut into small pieces, with fresh gastric juice at 100° F. for eight or nine hours. The filtered fluid, which is perfectly clear, but rather more deeply coloured than at first, has increased in specific gravity from 1010 to 1020, or 1022; and, by the addition of six or eight times its volume of alcohol, becomes excessively turbid and opaque, like thin milk. It is true that all parts of the muscular tissue do not dissolve; and there remains constantly, at the bottom of the test-tube in which the digestion has been performed, a pulverulent, brownish sediment, consisting of the undigested portions of the meat. This is the case, also, with boiled white of egg. While the digestion is going on, it is easy to see that as the general mass of the fragment becomes transparent and gradually liquefies, there remain, scattered through its substance, numerous minute opaque spots which are not acted on; and which, when set free by the solution of the rest, produce an opalescence in the fluid, and collect slowly as a sediment at the bottom. Whether these refractory portions are afterwards dissolved by the intestinal fluids, or whether they are altogether indigestible, and destined to be discharged with the excrements, is at present uncertain.

The *quantity of albuminoid matter dissolved by the gastric juice* has been variously stated. The method which I have adopted to determine this point is the following: it was first ascertained that the fresh lean meat of the bul-

lock's heart loses by desiccation 78 per cent. of its weight; 300 grains of such meat, cut into small pieces, were then digested for ten hours in ℥iss of gastric juice at 100° F., the mixture being gently agitated as often as every hour. The meat remaining undissolved was then collected on a previously weighed filter and evaporated to dryness. When perfectly dry, it weighed 55 grains. This represented, allowing for the loss by evaporation, 250 grains of the meat in its natural moist condition; 50 grains of meat were then dissolved by ℥iss of gastric juice, or a little over 30 grains per ℥j.

From these data, we can form some idea of the large quantity of gastric juice secreted during the process of digestion. One pound of raw meat is only a moderate meal for a medium-sized dog, and yet, to dissolve this quantity (supposing the whole of it to be digested), no less than sixteen pints of gastric juice will be necessary. This quantity, or any approximation to it, would be altogether incredible if we did not recollect that the gastric juice, as soon as it has dissolved its quota of food, *is immediately reabsorbed*, and enters the circulation with the alimentary substances which it holds in solution; so that a very large quantity of the secretion may be poured out during the digestion of a meal at an expense to the blood, at any one time, of only two or three ounces of fluid. The simplest investigation shows that the gastric juice does not accumulate in the stomach in any considerable quantity, to remain there until the solution of all the food has been accomplished, but that it is gradually secreted so long as any food remains undissolved; that portion which has been already digested being disposed of by reabsorption with its solvent fluid. There is, then, during digestion, a constant circulation of gastric juice from the vessels to the stomach, and from the stomach back again to the vessels. Or perhaps it would be more correct to say that it is only the watery portions of the juice, holding sometimes in solution the digested albumen and albuminose, that perform this circulation; while its acid and organic ingredients remain very possibly in the stomach, ready to act on a new quantity of food, as that which has been already digested is withdrawn by absorption. That this is really the case, is proved by the following facts. First, if a dog be killed some hours after taking a meal, there is never more than a very small quantity of fluid found in the stomach, just sufficient to penetrate and smear over the half-digested pieces of meat; and, secondly, in the living animal, gastric juice, drawn from the stomach when digestion of meat has been going on for six hours, contains little or no more organic matter in solution than that extracted fifteen to thirty minutes after the introduction of food. It has evidently been freshly secreted; and, in order to obtain gastric juice saturated with alimentary matter, it must be artificially digested with food in test-tubes, where this constant absorption and renovation cannot take place.

The time necessary for the digestion of food varies apparently in different species of animals. In the dog, one pound of lean uncooked meat is completely digested, and disappears from the stomach, only in from eight to nine hours. From experiments of M. Colin, of the Veterinary School at Alfort,

it appears that the process is equally long in the cat; and also, probably, in all purely carnivorous animals. Dr. Beaumont, on the contrary, found that in Alexis St. Martin, meat and bread were digested in five, four, and three hours; and even, in some instances, in an hour, or an hour and a half. This difference is undoubtedly in part owing to the influence of mastication, which is so very imperfectly performed in the carnivora. It might be supposed that, having constant opportunity of obtaining gastric juice from the dog, and of studying its effects in artificial digestion, we might easily determine the relative digestibility of the different articles of food used by ourselves. This, however, is not the case. The readiness with which a substance dissolves in gastric juice is only one element of its digestibility in the stomach. This depends also on the readiness with which it excites the secretion, and stimulates the peristaltic movements of the organ, the ease with which it disintegrates, the amount of previous mastication, &c. For the human species, then, the digestibility of various kinds of food can be properly determined only by direct observations on the human stomach, like those of Dr. Beaumont; and it will probably be long before we shall have any others so valuable, in this respect, as his.

Amylaceous substances are not digested in the stomach. It is easy to ascertain that cooked starch, artificially digested for ten hours in gastric juice, at 100° F., still exhibits its characteristic reaction with iodine. Bernard also maintains that it is equally unaffected in the stomach. Some writers, however, have thought that the well-known property of saliva in converting starch into sugar, would be exerted even in the stomach during the process of ordinary digestion. Lehmann, for example, states that "sugar may be detected, by the ordinary means, in the stomach of the animal (with gastric fistula) in ten to fifteen minutes after it has swallowed balls of starch, or after they have been introduced through the fistula." I am at a loss to understand the grounds of this assertion, as I have never met with similar results. Either cooked or raw starch, administered mixed with meat or introduced alone through the fistula (the animal will not eat pure starch) is easily recognizable by its reaction with iodine, ten, fifteen, and thirty minutes afterward. In forty-five minutes it is diminished in quantity, and in one hour has almost invariably disappeared; but no sugar is to be detected at any time. Outside the body, too, gastric juice entirely prevents the saccharine conversion of starch by saliva. A thin solution of boiled starch, mixed with an equal quantity of saliva, and kept at 100° F., is completely converted into sugar in ten to fifteen minutes; but if an equal quantity of fresh gastric juice be previously added to the mixture, there is no trace of sugar, even at the end of three hours, and the starch retains its usual properties. It must be acknowledged, therefore, that though saliva converts starch outside the body, when digested alone in test-tubes, yet it does not have this effect, so far as we can ascertain, during the natural process of digestion in the stomach.

Cane sugar is slowly converted by gastric juice, outside the body, into

grape. Ten grains of cane sugar, dissolved in half an ounce of gastric juice, and kept at 100° F., give traces of glucose at the end of two hours, and in three hours the quantity of glucose is considerable. It cannot be shown, however, that the gastric juice exerts this effect on sugar during ordinary digestion. If cane sugar be given to the dog while digestion of meat is going on, it disappears in from two to three hours, without any glucose being detected. There is a singular peculiarity, however, in the action of cane sugar, introduced into the stomach of the dog while empty. If half an ounce of loaf sugar be given to the animal after a twelve hours' fast, when the stomach contains no food or gastric juice, it almost invariably produces an immediate reflux of intestinal fluids (among which the bile is readily recognized by its color), and these promptly convert a part of the sugar which has been swallowed, so that in fifteen to thirty minutes the fluids extracted from the stomach contain an abundance of glucose. This, however, is only temporary. In forty-five minutes to an hour the intestinal fluids cease to be present, and the glucose at the same time disappears. Unaltered cane sugar, however, still remains and continues present for two and a half to three hours, when it gradually disappears without any subsequent production of glucose. During all this time, as has been mentioned above, no gastric juice is secreted; the reaction of the stomach remaining constantly neutral or alkaline.

The above experiments render it probable that even when cane sugar is taken with other articles of food, it is not converted into glucose in the stomach. We cannot, however, be positively sure of this, owing to a circumstance not generally known, viz. that *the presence of gastric juice interferes with Trommer's test for grape sugar.* If a drop of honey be mixed with a drachm or two of gastric juice, on adding sulphate of copper and potass, the solution takes a rich purple tinge instead of its ordinary blue colour, and, on boiling, changes to yellow—but no copper is deposited. A mixture of honey and gastric juice, in equal volumes even, does not reduce the copper unless it be previously diluted with water; in that case the copper is deposited as usual. I have not been able to ascertain upon what ingredient of the gastric juice this singular property depends. It is not its free acid, since, if previously neutralized by potass, it still behaves in the same way. It is not its organic ingredient, since if this be separated by boiling and filtering, it continues to act as before. Even the addition of several times its volume of alcohol does not altogether prevent the interference. A considerable proportion of glucose may still be recognized, notwithstanding the presence of gastric juice, by the change of colour from purple to yellow above mentioned, though there may be no precipitate; but a small proportion does not show itself in any way. One sixteenth of a drop of honey in one drachm of gastric juice produces no change of colour that could be at all relied on as a test; while the same proportion of honey in water reduces the copper promptly and abundantly. We cannot, therefore, detect a moderate quantity of glucose in gastric juice by this means.

It is universally agreed that oleaginous substances are not attacked by gastric juice, but are simply set free from their union with other matters. Thus, if adipose tissue be digested artificially, the walls of the fat vesicles, with the intervening areolar tissue, are dissolved, and the oil collects as a distinct layer on the surface. The oily matter of cheese acts in the same way, as already noticed. Boiled yolk of egg is digested in a similar manner. Its albuminoid ingredients are dissolved out, while the fats collect at the top of the solution, as a mass of yellowish semi-solid granules.

ART. IV.—*On Vital Force—its Pulmonic Origin and the General Laws of its Metamorphoses.* By GORDON HAKE, M. D., late Physician to the County Hospital, and President of the Young Men's Institute, Suffolk, England.

I. OFTEN as the attempt has been made to construct a rational theory of life, and frequently as its results have failed to satisfy the cravings of philosophy, this thing is certain—the efforts of physiologists to resolve the greatest of problems cannot cease so long as science itself lasts. The least fruitful works on this subject have served a purpose; they have paved a way for succeeding labourers. Even partial success was impossible at first; but now that the light of science has been admitted into every path of natural inquiry, views in strict conformity with its progress have sprung up, and, at this period, they almost yield a promise that the knot is to be untied. Of the essential nature of life itself, no more is understood now than was known in the earliest times; nor is it a subject on which speculation can be beneficially employed.

Our improved knowledge of the relations that physiological and physical science bear to each other, every day affords new facilities in the pursuit of inquiries into the living world. It has long been supposed that electricity is the active agent of the muscular and nervous functions, and experiments of the most varied kind have tended to confirm such an idea; but the hypothesis could not, until recently, have been said to have stood its ground. If at one time apparently established beyond all doubt, it has been set aside at another by arguments of the highest order. Not to pause, however, on the threshold of the present inquiry, by entering upon the history of this branch of science, it may be stated at once that Du Bois-Reymond, above all others, has finally put an end to doubt on the subject. Until the question of the electric origin of vital force had thus been settled, it would have been in vain to have proceeded with the discussion, contained in these pages, of the principles that such a question involves. The inquiry, thus as it were set free, becomes a wide one; and, to be conducted successfully, it must be extended not only to the relations of physical and vital phenomena, but likewise to the healthy and disordered states of the animal economy. The science of health affords a

clue to the phenomena of disease, and no surer proof of the accuracy of our notions can be adduced than that afforded us by testing the functions of health, so to speak, by their pathological reagents; or, in other words, by asking to what extent the properties of the one are adequate to explain those of the other. In clinical practice, such tests are found frequently to fail. This is especially true in reference to the dominant theory of animal temperature, a theory which fails signally to respond to the phenomena of heat that crowd disease, as well, indeed, as to the facts observable in the healthy condition. Now, since it is attempted in the following pages to show that vital force is a phenomenon dependent on respiration, and that animal heat is immediately consequent on the former and not on the latter, I deem it advisable, as a preliminary, to place the phenomena of animal temperature in such new point of view as the facts appear to warrant. For in the sequel it will be shown that, to acquire a correct notion of vital phenomena, it is above all necessary to know precisely what changes take place in the lungs during respiration, and in what order of sequence the temperature of the body is evolved, whether mediately or immediately, as the result of the pulmonary function.

The slow oxidation of carbon and hydrogen—elements rejected from the circulation—with the oxygen of the atmosphere, affords the only satisfactory account of whence animal warmth is derived. But beyond the discovery of this, its primary source, I do not hesitate to state that the subject of the temperature peculiar to warm-blooded animals has not been argued logically; the law through which ordinary substances are heated has been applied to animal bodies to explain their warmth, while that through which cooling takes place has been overlooked.

II. The well-defined laws of caloric teach us that a body like that of animals, if simply heated to a temperature of 100° F., and placed in a medium, like the air, of a lower temperature, would begin and continue to part with its heat to the latter, until the two reached the same thermal standard. For example, when the thermometer stood at zero, the human body, if mechanically heated, as is supposed, would sink rapidly in warmth until its temperature reached the same degree as that of the air, and like any other substance it would, under such circumstances, become frozen. The existence of a persistent source of heat to the blood, found in the slow oxidation of combustible matters within the range of a respiratory system, would not prevent the occurrence of this fatal accident to the system from cooling; the physical law would operate unless counteracted by the physiological; by a process calculated to preserve the thermo-genetic results of oxidation from the depressing influence of a lower temperature, such as almost uniformly exists in the atmosphere we live in.

Even if the respiratory process were adequate to sustain so constant a struggle for warmth against the effects of a lower external temperature, it is evident that a waste of heat would be constant, a large proportion of it would undergo radiation, and thus be lost to the economy.

III. But it should be apparent to all, who have viewed the subject reflectively, that no such cooling process does go on, except in disease or during sleep; and that the living body, in sustaining its high temperature in the midst of cold, contrary to the ordinary laws of thermotics, possesses a capacity for thus preserving its heat due to certain structures; not to the rate at which heat is generated in the circulation on the one hand, nor to the non-conducting property of the atmosphere on the other.

IV. The living body, however, under peculiar conditions, is equally liable with inorganic bodies to undergo the cooling process. This happens when it is placed in a medium of which the temperature is lower than its own, at the same time that it suffers under certain deviations from the standard of mature health, which may be thus arranged: 1. The state of infancy and old age; 2, injuries and affections of the nervous centres; 3, other forms of disease, to be specified.

V. In young animals, especially those whose nervous system is least perfectly developed, and in whose relation with the parent there is an established dependence for warmth, as in birds, the temperature of the body falls rapidly on exposure to cold; a law to the operation of which is to be traced the greater mortality of infants during the first months of existence. At a later period of life (and even in middle age, when the nervous system has been enfeebled by disease or by long-continued exertion), there is a return to the same physical law, the power to resist the influence of cold decreases.

In injuries and affections of the nervous system the physical law is observed, equally, to predominate over the physiological; depression of animal heat being found in constant relation with lesions affecting the nervous centres. Respiration, the primary source of animal heat, is found to fail in producing warmth, though the carbon of the lungs may be oxidized, whether by natural or artificial means.

Again, in those whose nervous system has been enfeebled by improper food, by prolonged muscular or mental exertion, by watching, or other cause of exhaustion, the cooling influence of the atmosphere is forcibly felt, and further, during sleep and hybernation, when a diminution of nervous force exists, a similar result obtains.

In other forms of disease besides those alluded to already, the temperature of the living body is observed to obey the physical law. In cholera, which may be adduced as a forcible example, this is strictly true; the warmth of the body in this affection has been known to sink as low as 67° F. The state of the system in this disease is that of collapse, a condition not dissimilar to that of shock from nervous lesions, and from loss of blood. The body is cold; the change effected in the circulating fluid by respiration goes on, but the temperature of the breath, tongue, and surface is equally depressed. The consequence of such collapse or nervous exhaustion is, that the living body is unable to maintain its natural warmth, though the carbon and hydrogen of the lungs is oxidized in the usual manner, and the product evolved.

VI. It appears, then, that the nervous system, if not solely, is specially necessary to the sustentation of a due warmth in animal bodies, and that it establishes in some manner a physiological law that regulates the heat of the system, and protects it from the physical influence of a lower temperature without.

It is the purpose of this paper to discover the law in question.

VII. It has been asserted above that beyond the discovery of its source in the chemical changes going on in the lungs, the temperature of animals has been unexplained, the law through which ordinary substances are heated having been applied to account for its distribution through the medium of the blood, without taking into consideration that the cooling process would in such case rapidly follow. For the general opinion of physiologists is, that heat resulting from the combustion of carbon and hydrogen in the lungs is communicated to, and diffused through the system at large, by means of the arterial current. But we know that if there be collapse, whether from loss of blood, or lesion of the nervous centres, or whatever other cause, the chemical actions, though still advancing in the lungs, are of no effect; caloric is not evolved. How is it that this condition of the system should be attended with such a result? Neither the blood nor the nervous structure can be supposed to possess any peculiar capacity for heat, for they cool with the same rapidity as other constituents of the body when the influence of the physical law is brought to bear upon them.

VIII. The theory of a temperature derivable from a mechanical diffusion throughout the body of heat generated in the lungs, is untenable as a whole, it being fraught with numerous errors of detail. In the first place, although no proof exists of the unequal conductibility of tissues, much less of parts of tissues, it is ascertained that the temperature of these is unequal and variable. The left ventricle of the heart is a degree warmer than the right, and a voluntary muscle is one or two degrees warmer during contraction than it is when in a quiescent state. The condition of the rectum, as respects temperature, is four degrees lower than the left, and three degrees lower than the right ventricle—that of the mouth is relatively the same. The hands are one degree and a half warmer than the cheeks, five degrees warmer than the groins and axillæ, three and a half warmer than the feet; and, lastly, arterial blood exceeds venous in temperature by one degree on Fahrenheit's scale.

All the tissues of the animal body equally are imperfect conductors of heat; it cannot be shown that any portion of a tissue, such as the skin, is a better or worse conductor than another, the skin of the hands, for example, than the skin of the feet. Therefore, the physical law which governs the distribution of caloric in common substances, from particle to particle, equally, throughout the entire volume, gives no explanation of the phenomena above cited as peculiar to living bodies.

These considerations render it necessary to reject the aid of the physical

law in explanation of the phenomena, and to inquire into their bearings in relation to a physiological law, hereafter to be explained.

IX. The influence of the nervous system in the generation of animal heat, suspected from the time of Haller, has not been explained. The physiological law which regulates the evolution of such heat is not discoverable in the nervous system alone; it is to be sought in its relations with the circulation of the blood. A lesion of the nervous system, and loss of blood, are equally opposed to the production of heat; collapse of animal warmth consequent on either, is due to the altered relation of the nervous and circulating systems to each other, to the suspension of their free contact, and thence of certain results of such, which will be duly described.

While the chemical sources of heat are on the one hand cut off on the occurrence of loss of blood, the power of converting these to their final purpose is destroyed on the other when the nervous system has suffered injury. The law of Chosat, based on a series of experiments, and on the earlier views of Brodie, may be here cited. It is, that the depression of animal heat is constantly in relation with lesions of the nervous system, whether they implicate the cerebro-spinal axis and its branches, or the system of the great sympathetic.

X. It remains to be asked, then, in reference to the well-known variations of animal temperature, to what extent it is possible to identify these with coincident conditions of the nervous function. If a correspondence can be traced, the relation of heat to nervous force becomes a less difficult problem to resolve.

The voluntary exercise of the muscles, which affords a good example of a nervous function, is accompanied by the evolution of heat. On this point, MM. Becquerel and Breschet performed well-known experiments with the thermo-electric multiplier; the arrangement was such as to indicate the temperature of the muscle in which the needle connected with the apparatus was inserted.

The needle being introduced into the biceps muscle while the arm was extended, it was found that on bending the latter, so as to contract the muscle, the deviation of the magnetic bar (which was proposed to indicate whatever changes in the temperature took place) was increased suddenly; and on the arm being bent anew several times in succession, a final deviation was observed equal altogether to nearly two degrees F.

This result is a parallel instance to that which occurs during involuntary muscular action; the temperature of the heart being higher than any other part of the body—and on the occurrence of rigor the depressed temperature of the skin being finally raised to fever heat, by the involuntary muscular contractions which attend it.

Thus, the temperature of a part rises at the same moment that nervous or its equivalent muscular force is expended.

An eminent authority (Dr. Edwards) holds the opinion that the increased

temperature of muscles, at the moment of their contraction, is due to an afflux of blood to them; but it is a fact, which I have witnessed myself, that, during the shortening of contractile fibres, the blood is expelled from their vessels, and their colour is lost.

XI. Increase of temperature accompanies the afflux of blood to a part; not when this is occasioned by mere mechanical impediment to its circulation—for then the blood, to whatever extent it accumulates, is of the same degree of heat as when moving in a natural current—but only when such remora is caused by the exercise of nervous functions. As examples of this may be instanced blushing, the nervous action attendant on which is emotion; and inflammation, of which the nervous accompaniment is pain.

XII. It is apparent, then, that nervous, as well as its equivalent muscular force, contributes to the operation of that physiological law which governs the distribution of animal heat, and that the integrity of the circulation is essential to its activity. The blood passes freely to the nervous structures, its course through the capillaries gives it contact with the cerebro-spinal axis, and, while a sixth part of that fluid, perhaps, is constantly traversing the brain, the whole returns to the lungs.

Thus, while in the lesser circulation those calorific phenomena commence by means of which the temperature of the body is sustained, it is due to the relation of the greater circulation and the nervous system to each other that the evolution of heat is finally effected.

XIII. The tendency of the present argument being to demonstrate that the nervous structures, though they do not take the initiative in producing animal heat, inasmuch as they do not supply the materials whence it originates—constitute the sole organization through which it is, during health, evolved and distributed, it has to be determined what is the *modus operandi* of the nervous influence thus exercised—of the physiological law?

If heat, intimately allied to the chemical action of the atmosphere on the lesser circulation, is appreciable to the system only through the instrumentality of the nervous centres, it cannot but be allowed, by those who have followed the recent developments of physical science in relation to physiology, that electricity is the form in which those centres contain the antecedent of such heat. The metamorphosis of the one into the other is of common occurrence, and easily illustrated by the experiment of the voltaic arc; but the difficulty of the subject does not consist in the phenomenal changes, so much as in the order of their succession; and for this reason I propose to trace the connection between the oxidation of carbon in the lungs and the presence of electrical currents in the nervous system.

XIV. It is an admitted principle that electricity is chemical force acting at a distance; this is easily proved by experiments; and there can be little doubt, if the question be duly considered, that the electricity of the nervous system represents the chemical force of which the lesser circulation is the seat, and the oxidation of carbon is the main source. But there is an undiscovered link

in the chain of phenomena; in the physical world this link is supplied by the electrode, but in the animal it has failed of being detected.

Still, throughout the vegetable and animal kingdoms, electrical phenomena are universal; organic conductors, therefore, exist in living bodies; these are constituted of the cells. The nervous and muscular fibres present well-known examples of conductors, and may be cited as having been viewed as such by the great experimenters on the subject of animal electricity.

The earliest and safest induction, therefore, that can be made in the research —that of the discovery of the electrodes connecting the chemical and electrical forces of the economy—is derivable from the study of cell-life itself, and from a comparison of the functions of the fixed cells of organs and the free cells of blood. In pursuing the subject through this channel, I intend to adhere to physical reasoning; being convinced that, whenever such is lost sight of, the path of truth recedes, and is only again recovered after a fruitless delay.

The cell has been truly characterized as the type of organization. All phenomena pertaining to cell structure are resolvable into the individual and relative actions of these. The result of their individual actions is their vitality; that of their relations is the vitality of some whole, such as a fibre, an organ, or an entire being. The individual life of a cell consists in the exercise of its self-nutrient office; the relative life, is the part it performs in that union of its own species which forms a whole or aggregate life. The act of nutrition in the cell is a chemical process; it consists in the conversion of one material into another; of certain elements of blood, for example, into milk or bile, and of their absorption into the secreting cell; of others, into nerve-cell or muscle-cell. Whether the metamorphosis takes place from blood into the material assimilated by the cell before or during absorption, is of no moment; the result—viz. that the assimilative act is affinitive—is the same, and indicates the exercise of chemical force.

This force, whether manifested within the cell individually, or as the united action of many cells—that is, whether acting at the seat of the changes which occur, or at a distance—is still due to chemical transformations; only, in the one case, it is called chemical, in the other, electrical force.

XV. There is only one type of cell; its forms vary, their figure and development corresponding with the place they occupy in the organization, and the office they have there to perform. Each cell, thus depending for its office on its position, has a determinate energy; the measure of this may be, in one instance, its self-multiplying force; in another, its motor; in a third, its secreting power; but the cell that reproduces itself, as in the rapid growth of the embryo, does not fulfil any secondary office in the economy, while those which perform secreting and motor functions do not reproduce themselves, but, when exhausted, are replaced by a new birth. But there is a variety of cell in the system, whose function has not been determined, the red corpuscle of the blood. All cell-structures in the vegetable and animal kingdoms are referable to an archetype, and are a proportional of such, which may be said

to consist of a cell-wall and nucleus; all contain these two elements in uncertain proportions, or one of them only. In secreting cells, for example, the nucleus is the predominating element; in those devoted to the formation of solid structures, the cell-wall; while in the free cells, or red corpuscles of the blood, examples are found of nucleated cells fully formed, as in the frog, proteus, and siren; and of cells without nuclei, as in the mammalia generally.

On the grounds of analogy, I am led to conclude that the free cells of the blood perform individual and aggregate functions like other cells, and that their office also is determined by their position in the economy.

This position is unique; it is one that exposes the cell in question to the atmosphere, and, therefore, to chemical influences, while the circumstance of its being free, points to some peculiar office that it has to fulfil.

XVI. The free cell of the blood, like any other single floating cell, is a monad, the determinate force of which, from the period of its full growth, is expended on the respiratory office; it is, essentially, a breathing cell. All cells, in a less degree, partake of this character; those approaching nearest to it are certain forms of zoophyte, which inhabit the waters. More than this, when the nature of nutrition is considered, the resemblance of cell-function is made to appear more general; all cells inhabit a medium, whether it be the waters of the globe, or the liquor sanguinis—for both the fixed and free cells occupy the latter in living bodies; and, within this medium, chemical changes, in all instances, transpire. These chemical phenomena occur universally between the cell and the medium it inhabits; between the bile, or the milk-cell, and the blood; between the monas, or the red corpuscle, and their media, together with circumambient atmosphere. The chemical phenomena of nutrition transpire in the former instances; those of nutrition, but especially of respiration, in the latter.

XVII. It is of no moment, as affecting the present argument, whether the office of a cell be that of development, secretion, or respiration; in each of these relations alike the result is chemical, a result which assimilates the functions of cells, and proves, which was to be demonstrated, that the free cell of the blood, like the fixed ones of the system, is a centre of chemical changes, and, as such, an electrophorus, like them.

The chemical changes which characterize the nutrient office in algæ and confervæ, in bile-cells, or nerve-cells, do not result in animal heat, but in another result of chemical action, electricity; a force which, when manifested thus in organic media, is denominated vital.

On the same ground, the result of chemical changes as they occur in the free cells, the oxidation of hydro-carbon, is not calorific, but electric.

XVIII. It has already been shown how inadequate in itself the oxidation of hydro-carbon in the lungs is to maintain the temperature of animal bodies; that though such should proceed with its wonted energy, the system cools rapidly when an injury occurs to the nervous centres. It was necessary, therefore, to inquire by what law the force generated at the lungs was con-

served, so as to be ultimately manifested in the form of caloric in the system at large; and the result of this inquiry, as far as it has proceeded, tends to show that such force, having a chemical origin, is rendered electric through the medium of the red corpuscles, or free cells of the blood. Such a conclusion, as I have shown, is in strict analogy with the physiology of cell-life; by pursuing the subject further, it may be seen that certain structures and elements belong to those red corpuscles which must, necessarily, convert caloric, should such be evolved, into its equivalent of electric force; a fact of deep interest in the study of cell-life at large.

XIX. The free cells of the blood, or its red corpuscles, are not homogeneous in structure, nor simple in their composition; in the first place, they are built up of hæmatin and globulin, substances of different densities, the surfaces of which are opposed; and secondly, they contain free metals, iron and potassium with sodium, besides salts of others, magnesia and lime. Now, either of these conditions is sufficient to convert chemical into electric force, without its passing through the intermediate form of caloric.

The experiments of Seebeck and others on thermo-electricity have demonstrated that all substances are capable, more or less, of converting heat into electric force. A common illustration is derived from two bars, one of bismuth, the other of antimony; these are soldered together at one of their extremities, to which point of junction a spirit-lamp is applied, while wires are attached to the opposite ends, which, by this means, may be connected with a galvanometer. On the connection being completed, the evolution of electricity is manifested by the deflection of the needle.

Any other metals will serve for the experiment, or two different surfaces of the same metal.

But instead of caloric, chemical action may take the initiative in the generation of electric force, without sensible heat being manifested. In the galvanic battery, the chemical action of the acid upon the metals causes heat to be evolved, but when the circuit is completed, heat ceases to be developed, and electricity is produced in its stead. Through the medium, therefore, of the blood-corpuscles, in which the requisite conditions are embodied, the chemical action of the atmosphere, or oxidation of hydro-carbon within the lungs, is convertible into electric force without passing through the intermediate form of heat.

XX. The free cells of blood are always in contact, from the heart to the capillary vessels, and form, therefore, continuous chains; thus, like the fixed cells of the nervous and muscular systems, they become conductors.

These conductors are the electrodes which connect the lesser circulation, or centre of chemical action with the great nervous centres, through the medium of the greater circulation.

Electricity is chemical force acting at a distance, or, in other words, manifested through a new medium, the electrodes, at the same time that the exact equivalent of force, from which it is derived, is preserved.

Thus, then, the entire equivalent of chemical force, without being converted into heat at this stage, is conveyed to the remotest parts of the system, secure from the possibility of loss through the cooling effects of exposure to an atmosphere of lower temperature than the living body.

XXI. These premises established, it is to be perceived that, except in disease, a constant supply of electricity is derived from the action of the atmosphere on the hydro-carbon of the blood; this is metamorphosed into heat at every point of the system, by a process as simple as that which converts electricity into heat, on the electrodes of the battery being brought into contact with each other.

It will be shown, however, in the succeeding paragraphs, that the electricity, thus derived, is changed into vital force on reaching the cerebro-spinal centres; and in its passage from these to the muscles, the method of its manifestation is yet further modified; that, in fact, it goes through its circuit of metamorphoses in traversing the different organs of the body along the courses of nervous fibres still retaining the same general characteristics, and that, finally, it is converted into caloric on the completion of a vital act, such as that of sensation, muscular motion, secretion, &c.

Many experimental inquirers, from Faraday to Du Bois Reymond, have insisted on the identity of electrical and vital force; none of them, however, have doubted the received opinion, which I shall prove to be fallacious, that the great nervous centres generate their own vital force. They have not, indeed, even started the question as to whether the white or the gray substance of these centres is instrumental in generating the force under discussion.

XXII. The nerve-structure is adapted, on dynamic principles, to subserve its telegraphic purpose of transmitting impressions; though not without the intervention of a force. To put the question in this form may appear startling at first sight, but, by examining it in the manner I propose to do, it will be found to contain materials for an analysis, the results of which are novel. Of the nerve itself, a simple membranous tube containing the nervous substance, it need only be premised that its semifluid medulla, so easily affected by agents that even water disturbs the arrangement of its molecules, appears to be suited to its telegraphic purposes in a special manner. This may be in some measure shown on common dynamic principles, the medulla, from its semifluid consistence, offering a medium of great mobility for the transference of an impression from one end of its track to the other. To effect the necessary change in its molecules, what would appear important in the process is, that they should be susceptible of a perfectly equable disturbance throughout their course; in a sensational nerve from the extremity to the point at which it makes contact with a central cineritious structure, and in the opposite direction in a motor. The medulla is the organ of these polar changes, which could not, however, be effected without the intervention of a vital force, any

more than the molecular equilibrium of a bar of magnetic iron could be disturbed without an inward force to take up the external impression.

XXIII. The vital force residing in nerves, and cumulative around their molecules, is not apparently inherent in their structure, but is derived by them from some central element of the nervous system. It does not strike any inquirer as even probable that nerves generate their own force; disease of the nervous centres confirming a directly opposite view by the paralysis of the branches. Upon this fact rests the solution of the whole problem; for, if the nervous fibre is restricted to the reception and transmission of currents, so also is the nervous fibre of the medullary portion of the spinal cord and encephalon. In these nervous centres the white matter has the same structure as nerves; both have a tubular envelop, a soft and pulpy consistence. The tubes are not bound together in the brain and cord by areolar tissue, as in the nerves, nor are they arranged in the same order; there is no other difference between them. Of such tubes, without intermixture of gray substance, a large portion of the central nervous masses is formed; indeed, the entire medullary structure of these organs. Two sets of the white fibres in question are traceable as continuous with the afferent and efferent nerves, so as to form one structure with them; and the large remaining series is commissural; they must, therefore, having the same structure and relations as nerves, perform analogous functions, and derive their vital force from the same source.

XXIV. The gray substance, then, is the only element among nervous masses that remains to be considered as the generator of a vital force. If the nervous centres include a structure to which this force is due, it must necessarily be the cineritious; since the medullary structures are tubular alike in the nerves, the cord, and the encephalon; are conductors of nervous currents, but not the seat of their generation. Is, then, the vital force originated in the cineritious substance?

XXV. Pursuing the same line of investigation, that of considering function in reference to structure, it is of importance to state in this place what are the characteristic features of the gray substance, and what its relations. The gray and white matter of the nervous centres, including ganglia, have no structure in common, though blended one with the other. The former contains none of the fibres of which the latter consists, but is composed of cells with nucleolated nucleoli and granular contents; of caudate nerve vesicles, and pigment particles. But for their contiguity and intimate union, the gray and white matter, therefore, might be viewed as distinct organs, so dissimilar must be their functions; indeed, they resemble each other less in structure, and probably in function, than the gray matter resembles the red corpuscles of the blood.

But the nerve-tubes are everywhere imbedded in the gray substance; the phenomena, therefore, which attend its functions are in strict relation with them, though a total dissimilarity of action must characterize the uses of the two.

XXVI. But it is necessary to proceed further in the analysis before attempting to settle the question, whether the gray substance of the nervous centres is capable of generating a vital force. In the first place, since all forces, whether vital or physical, are equivalents of each other—that is to say, since a certain sum of chemical can be metamorphosed only into a definite amount of electrical force—it is a matter of rigid import to ascertain whether an organ supposed to originate a force by means of its own structural changes —that is, through the electrolysis of its materials—is physically capable of the result attributed to it. With this view, I have endeavoured, approximatively, to arrive at the relative proportions of the gray and white substances of the encephalon; and, by a dissection, performed with as much accuracy as possible, on a brain weighing 20,888 grains, I obtained the following result:—

Weight of the encephalon	. . . .	20,888	grains.
" " gray substance	. . .	13,000	"
" " white substance	. . .	7,688	"

XXVII. The chemical analysis of the brain also contains facts illustrative of the preliminary inquiry, and these must be studied before the induction can be fairly made. While these show a less composite structure in the cortical than in the medullary substance (the facts being in a great degree absent in the former), the composition of the granular matter appears, in some respects, to resemble that of the colouring matter of the blood. But the fact of the highest import to the present question is, that water is the principal constituent of cerebral substance, the proportion it bears to the solid materials being from 70 to 88 per cent. This very expressive fact is not in the slightest degree inconsistent with the functions attributable to the medullary structures, as conductors of a vital force, but it would have a different bearing in relation to the cineritious substance viewed as the generator of such a principle. It cannot be supposed, for a moment, that the electrolysis of water within the encephalon yields electric currents which become vital under new media of manifestation; such currents, therefore, if taking their source at all in the brain, must originate in the solid residuum of the cineritious substance, and this would amount to one-fifth part of it, as a mean, or 2,640 grains.

Such an amount of solid matter, were it even possible for the whole of it to undergo electrolysis daily, would be inadequate to the purposes required. The generation of a vital force within so limited a sphere would leave no space for the true functions of the gray matter, such as the exercise of will, and other mental phenomena, for it would necessitate a ceaseless and rapid renewal of structure. In a word, were the generation of vital force to be demanded of so limited and delicate an organization, its tissue must undergo destruction in one day.

Let us for a moment reflect on the vast amount of muscular, nervous, and mental force that is expended daily in health; how much thought, speech, emotion, and bodily labour may be endured; and then ask the question whether

the electrolysis, not of between two and three thousand grains of solid matter, for such would involve the total destruction of the gray substance, but a portion of this, affords an adequate explanation of the phenomena.

XXVIII. It cannot but be perceived that the nervous system is absolutely wanting in structures suited to the production of its own force; and that, therefore, it does not present an exception, as was supposed, to the law that no organ generates its own nervous currents. Indeed, it is difficult to conceive that nature would so deviate from her economy as to use the materials of her most perfect organisms as electrolytes, when the detritus of the system, composed of the same materials, was at her command, and equally available for her purpose. A hundred grains of carbon are every hour brought to the lungs for oxidation, besides the sulphur, phosphorus, and hydrogen, which unite in different parts of the system with oxygen absorbed during breathing; and these materials, beyond all reasonable doubt, afford the only source of the vital forces, whether nervous, electric, or calorific, all of which are mutually convertible into each other, and have a common origin.

After careful consideration of the facts supplied by analogy, respecting cell-function, I am led to conclude that the red corpuscles of the blood are electrophorous, and that they are the probable media of conveyance for the electric force, arising immediately out of the oxidizing process, from the lungs and other parts of the system to the nervous centres.

XXIX. It being admitted, then, that the oxidation of carbon and other bodies is effected under circumstances that give rise to electric, and not, as was hitherto taught, to calorific currents, and that these are finally convertible into their equivalent of heat, it may be here observed that the theory which enables us to express such a result, contains within it the means of measuring these equivalents, by the calculation of the amount of oxygen absorbed, and of thus reducing the amount of vital force itself to a physical standard.

XXX. It is allowed, by all great authorities on the subject, that the currents observed in nerves and muscles, on being manifested through the medium of the nervous centres, and which have an electro-magnetic action on the galvanometer, are identical with those produced artificially by means of the battery, and which have a similar action. This discovery, so long in gaining due ground, though urged by such names as Faraday, Brodie, and Wilson Philip, is finally confirmed by Matteucci and Du Bois-Reymond. There can now be no hesitation in assenting to the proposition that electric force, generated in the lungs in the manner already explained, becomes vital force on penetrating the nervous centres. And it appears to me even probable that the seat of this metamorphosis is their cineritious substance; for it is there that the huge volume of blood given to the brain is chiefly distributed, while the peculiar cells before described, irregularly arranged, are the better adapted to insulate and render static the vital force; in fact, forming a structure totally different from that of the medullary portion of the encephalon, by the instrumentality of which the conduction of will and thought proceeds actively.

XXXI. The electric currents of the red corpuscles, then, on reaching the capillaries of the vesicular substance of nervous centres, are metamorphosed, according to this view, into vital, but no sooner are they manifested as such through their new medium than a fresh transformation occurs; once more in motion, they attain a novel development. If they escape the nerve in a sensational form, or pass from it through muscle, the result is the same—the vital force becomes again electric before finally being metamorphosed into animal heat. These changes follow an order of succession, proved to occur by experiment and explicable by aid of physical laws. The electro-motive state of a nerve and muscle is one of vital force; when it acts, the force which is evolved is electric, as shown by Du Bois-Reymond; finally, nervous and muscular action, thus circumstanced, is followed by the evolution of heat—a fact observable in emotion and many other nervous phenomena on the one hand, and made sensible to the thermo-electric multiplier on the other. It is unnecessary here to detail the well-known process of a metamorphosis of electricity into heat; it is daily witnessed at the electrodes of the galvanic battery, when the arc is formed and the current is impeded.

XXXII. Regarding the laws of their metamorphoses in this light, it is interesting to observe how intelligibly a well-known series of phenomena, believed in some manner to have a mutual dependence, may be explained. The theory of combustion, for which I propose to substitute that of oxidation simply, was inadequate to account for the whole vital heat of the system; and, as chemists do not recognize the same presence of heat during the analytic process as is found to accompany the synthetic, they were unable to put down the destructive changes, which are perpetually advancing in the animal economy, to the account of its temperature.

The process of waste involves a large amount of decomposition; that of secretion is likewise preceded by a destruction of one organic form before another, or the proper compound, is formed; as recognizable, for example, in the production of an element of bile from the red corpuscles of the blood, its fibrin, and fat. But how is it possible to determine the equivalent of heat from these analyses, and of electricity from these syntheses of the animal body? By rejecting the theory of combustion (which really occurs only during disease), and adopting that of oxidation, the matter assumes a much more simple aspect; instead of having to formulate the calorific results of the assimilative and other synthetic processes, and the electric results of the various decompositions, it is necessary only to view every organic change as attended by the evolution of its equivalent of electric force.

XXXIII. The theory thus propounded of a vital force having its origin in the chemical changes of the system, sufficient in extent to sustain it constantly, and acting through the intermediate link of electricity, confirms the opinion entertained by physiologists that this force is manifested only at the moment of contact between the blood and nervous centres. The origin assigned it, however, in this essay, though differing from that attributed to it by other

writers, cannot fail to be admitted as correct, if the facts be well considered. Of the forces pervading living matter, there can be but one main-spring; in the mean time, the nervous centres bear no resemblance to organs employed in the generation of force; their electrolysis, when it occurs, is in connection with their vegetative function. For the rest, their utmost integrity is conserved jealously, in order that impressions may be retained, and reflections performed, at the same time that the material through which so great a modification of force transpires may, without further change than is necessary to assimilation, remain true to its purpose of effecting the highest metamorphosis in nature.

That any process besides that of waste, after the repeated exercise of polar functions, should be carried on in the brain or other nervous centres, will appear impossible to those who are able duly to estimate the nature of force; to such it would appear equally probable that caloric itself should be generated without a physical equivalent. Such argument admitted, we have only to weigh the phosphates daily ejected from the system, to adopt the conclusion that the nervous centres do not yield the physical resources through which their own currents are supplied.

XXXIV. The retina presents as good a means as can be selected for examining into the working of this new theory. An image reflected upon this organ is impressed only on the surface, it does not penetrate the strata of nerve-cells which intervene between this surface and the cerebrum, unless certain conditions are present, the chief of which to be mentioned, for the present purpose, is a circulation of blood through the brain. Let this be arrested, the image may be perfect, but it is not impressed on the nervous tract, it is invisible; let the circulation be restored, and the image is seen. This experiment demonstrates to reason that the optic tract does not possess within its own structure the means of bringing its molecules or cells into the same polar state as that of the retina when an image is reflected on the latter. The optic tract, then, is in the same condition as soft iron; this, encircled by the wires of a galvanic battery, becomes magnetic, and remains so as long as the electric current circulates around it. While in this state, the whole of the metal is in harmony with its poles, and acts through them; arrests the circulation of electric currents, and the unity of action among its molecules is at an end. The circulation of the blood is to the optic tract, what the circulation of electric currents is to the soft iron.

The same argument precisely applies to the brain, and to all the nervous centres into which capillary vessels enter. The cerebro-spinal axis, the ganglia of the sympathetic, are temporary magnets; arrest the current of their circulation, and their functions are paralyzed, for the obvious reason that the electricity conveyed to them by means of the electrophori, or free cells of the blood, is cut off, instead of, as heretofore, being metamorphosed into vital force, on traversing the nervous structures.

The individual functions of all nerve-cells, like those of others throughout the economy, are simply vegetative, self-nutrient; they are responsive to each other's impressions, not through their own vital force—which is solely that of affinity for the elements of nutrition—but through the polarizing force of that vital power which the circulation brings to bear on them as a whole, and in the manner explained.

XXXV. The vital force of the brain is analogous to the physical force of the magnet; it is not identical with it, inasmuch as cerebral and ferruginous substances, acting as their media of manifestation, are totally different in structure; yet the identity is so far complete that the same electric currents are competent to produce vital force in the brain, and magnetic force on soft iron.

The brain is an organic magnet of the highest order of its class; its properties are subject to modifications from the action of new forces upon it, in the same manner, and probably through the same laws, as the properties of an ordinary magnet may be modified by a new force, such as a current of electricity, which causes a magnetic bar to be deflected.

Vital force thus bears a stricter resemblance to magnetism than to electricity, its direct source.

XXXVI. Cerebro-vital force, when united in action with other forces within the same organic medium—namely, with light, heat, and motion (for the phenomena of sound, and the contact of bodies acting on the sense of touch, are resolvable into the latter force)—a new result obtains; there are observed phenomena of sense and intelligence.

The phenomenon of mind, viewed as the ultimate result of a composition of forces, has a certain physical resemblance to these, in as far as the perception of nature is similar, in most respects, to nature; except that the former, unlike the latter, is a consciousness.

In tracing results to their antecedents, it is not possible, in the simplest class of changes, to understand a cause; we only accept the evidence it affords. Oxygen and hydrogen produce water; but we perceive the essential nature of the change as imperfectly as we see what transpires when the most complex forces in nature become compounded, and are attended with consciousness.

It is, however, a step in advance if we can show that consciousness is a phenomenon, like all others, the result of antecedents.

The brain depends on a free current of vital force for the manifestation of its properties; if such force be withdrawn, the action of the extrinsic forces is enfeebled, in the same manner as the deflection of a magnet would be slight, however free the current of electricity in the direction of its poles, if such magnet had lost a portion of its power.

The brain is an induced, or temporary organic magnet, but the supply of its vital force is constant as long as the circulation of the blood, whence it is derived, is healthy. There are states of disease in which this supply is cut

off; I would instance anæmia. In this affection, the power of attention is singularly defective; there is forgetfulness; the action of the faculties is slow, and any attempt to hurry them in their work is productive of wildness; a weak state of mind characterizes this affection.

But whether the supply of vital force be cut off at the brain or at the lungs, the result differs but slightly; it signifies but little whether chemical force be checked in its evolution during the respiratory act, or whether its equivalent in vital force be withheld at the brain owing to disease of the free cells of the blood. In organic asthma, as in ænemia, there is irritation of mind, sometimes a clouded state of the faculties; the strength is prostrated in both cases, however powerful the muscles. The memory, in these affections, is characterized, not by inability to remember, but by want of vital strength to exercise itself. Though the emotions are less vivid, and the faculties show diminished energy, in none of these is lost a consciousness of their latent power—the static force inherent in the vegetative functions of the encephalon.

XXXVII. Any approach, however slight, that can be made towards resolving the grandest of all problems—the nature and sources of the mind—confers a lasting benefit on science. The materials through which the argument in the present paper has been effected, and the method of thought that has been followed, have suggested new views which may not be unworthy of perusal.

Every ascending step in nature is won from the rude mass by those truly wonderful combinations of matter which are due to the affinities. To limit these to the chemical range, would be to take too narrow a view; there are others, besides, which unite element with element; to them, in a more composite form, is due the attraction of world for world, and even the whole harmony of nature.

We see what these affinities will effect among the inorganic elements; but how much more they perform when they give rise to structure—to the living kingdoms! It is in these that we see the result of a composition of forces the most opposite in their origin, the furthest apart.

The forces, or many of them, have the power of modifying each other, and thus of producing a result unlooked for, and wholly different in character to the properties of those agencies which were engaged in its production.

Light modifies the union of elements, both crude and organic; it gives rise to peculiarities due solely to the addition of that force, yet not in the slightest degree evident in the force itself.

Heat, that is the temperature of 100° F., is a necessary condition to the performance of living functions; to the composition and decomposition of organic elements; it enters into the process of union and separation.

Electricity, under the aspect of vital force, gives a character of its own to vital changes; without it, they could not go forward, or, if they advanced, it would be towards the production of abnormal compounds.

It may be assumed generally, that the correlative forces—those which are

mutually convertible into each other—are capable of entering into combination, and of giving rise to a composition of forces, provided that chemical force, inorganically or organically derived, be present. The forces thus mutually related are, electricity, light, heat, and chemical action, to which may be added motion and magnetism.

The forces thus instanced are in themselves compound, and severally express the activity of other bodies. Chemical force is the composite of minor affinities; so, also, is electricity, light, and heat. Either and all of these may be generated by combustion.

Now what is remarkable in reference to these forces is that, though they are correlative, identical in their essence, and different only in their medium of manifestation, they are capable of combining, as will be shown presently, and of giving rise to a new result.

But though electricity, light, and heat will not unite together directly, they are capable of doing so through a medium—through materials capable of being excited to chemical action. Instances of such are witnessed universally; not only in the laboratory, but in the living kingdoms.

XXXVIII. But, to apply this great principle to the present object, let us examine its bearings on the nervous system—on the seat of mind.

We have seen what the retina is in itself; a mere organic mirror, a congeries of organic cells whose constitution is chemical, the office of each being to maintain itself in perfect integrity by means of nutrition, to thus repair waste as constantly as such accrues from the exercise of function.

Regarding the organic cell of the retina as an organic chemical basis—a medium for the manifestation of vital force—with no other function than appertains to it as such, let us inquire into the result of certain physical forces being added. To develop vision, neither electricity, heat, nor light, alone, nor any two of these forces, is sufficient; light is necessary to the living function of the retina itself; electricity to that of the tract between it and the sensorium; heat, or the temperature of blood, to both. The physical forces thus brought to bear, in the manner described, on an organic basis, give rise to a result totally new—vision; a vital force.

XXXIX. The composition of forces like this, accruing in organic media, occasions results proportionally important. The collective experience of all the senses—that is, actual vision, hearing, &c.—becomes so many vital forces, subject to the laws of composition and decomposition, in reference to the organs of intellect, emotion, consciousness, and volition, which, in their turn, become the chemical basis, or static medium, in relation to these vital forces, for the manifestation of mental force.

Without pursuing this deeply-interesting subject into too much detail, I would thus suggest it as probable that intellectual phenomena, like those of sight, are due to a composition of forces—to the last and highest of such as outward nature and the organism combined can contribute.

XL. Before concluding this argument in reference to the mind, I would venture to make a further suggestion, as to whether emotion and sensation may not have an origin the very opposite of intellectual phenomena. The latter are due, as a basis, to the white fibres; they are reflections along these to the seats of the faculties. Not so the former, they have their seat in the gray substance, and the glow accompanying them indicates a metamorphosis of force. The senses and intellect convey intelligence; when such has traversed the white substance, in which all dynamic actions of the mind occur, they reach the gray, or seats of emotional phenomena. Is not the glow which here occurs, and which frequently diffuses itself in the form of heat over the nervous system, a decomposition of the vital forces constituting the intellectual, and a metamorphosis of one of their penultimate elements—electricity—into animal heat?

This view of a composition and decomposition of forces—which are the telegraphic agencies of every complex operation in nature, as giving rise not only to the lower forms of organic function, but finally to the phenomena of mind itself—may be worthy of consideration to those whose study of physical and physiological science as one, have led them into a similar field of investigation.

XLI. The voltaic arc, by the completion of which the electric currents, instead of passing off silently at the electrodes, are impeded, and by the change of length in their waves, converted into heat, affords a parallel of what sometimes takes place in the animal economy, especially when inflammation is present. The stasis of the free cells of the blood during the inflammatory process not only prevents their polarization from taking place, but hinders the cells in adjacent vessels from moving forward. They oscillate instead of advancing, and, being cut off from the general current, their electricity is impeded, and consequently metamorphosed into heat.

The inflammatory process has been studied with much research, but there are points connected with it impossible to be understood before a rational explanation of animal heat had been arrived at.

Let us review the great local symptoms of inflammation, pain, swelling, redness, heat. A remora of the free cells of the blood within the capillaries, explains the second and third of these; they arise from obstruction, the causes of which are foreign to the general argument; the first and last of these symptoms, however, stand in a peculiar relation to each other, a relation similar to that of vital force and animal temperature at large.

In every system, the force destined to control organic functions, undergoes a metamorphosis; chemical in the lungs, it becomes electrical in the blood-cells, nervous in the cerebro-spinal axis and the ganglia of the sympathetic, endo-muscular in the muscles, and calorific, when, as nervous or endo-muscular, its vital act is completed.

It has been shown how disease, or deficiency of the blood-cells, gives rise

to deficient vital force in the nervous centres, and how impediment to the respiratory process conduces to the same result by preventing the oxidation of carbon within the lungs, and thus curtailing the source of electricity in the system.

When vital force is thus reduced in its amount, from whatever cause, febrile states of the system are apt to arise; these are easy of explanation on the views now brought forward.

The coexisting rigor and flush, accompanied by creeping sensations of chill and heat, are indicative of a low state of vital force, and of its imperfect and irregular conversion into caloric. The balance of function is destroyed between the nervous and muscular systems; vital force is expended within the latter by its involuntary movements, or rigors; but these convert it, according to the law already described, into caloric. When shivering has thus proceeded for a certain time, the metamorphosis of vital force into caloric has sufficed to cause reaction; a hot stage. By the time that this stage subsides into that of normal temperature, the function of the lungs, circulation, and nervous system, for the performance of which such temperature is required, are resumed, and the balance is restored, at least for a time. Unless the vital force is irreparably exhausted, as in cases of cholera, the cold stage thus gradually develops the hot; the closure of the pores prevents loss of heat by evaporation, while the trembling of the muscles gradually generates it.

A low degree of vital force from disease, as well as from other causes, such as incomplete development of the nervous system in the young animal, the physical condition of age, &c., thus infallibly occasions a corresponding low degree of heat in the animal body; and from the obvious cause that one is the antecedent of the other in disease, as well as health.

Any impediment to the free action of electric currents occasions heat, both in disease and health. The distension of the vessels in inflammation, impeding their currents by pressure upon the nervous branches, causes pain, and this must be an additional source of caloric; for whenever a vital act is completed, whether sensational or motor, the result is an evolution of heat. A paroxysm of neuralgia leaves the seat of pain deeply flushed; the same effect is frequently an accompaniment of emotion, in which example the blush of the cheek is the counterpart of the same remora throughout the gray nervous substance.

In inflammation, as in health, the impeded nervous current (pain, emotion) precedes the remora (stasis of free cells—permanent, transient).

In secreting organs, the vessels are so distributed as to produce remora of a normal kind; all the capillary vessels partake more or less of this distribution, which is no doubt in some manner connected with the maintenance of a certain temperature, the existence of which is necessary to the carrying on of the vital functions generally.

Inflammation of the lungs is attended with a degree of prostration which accompanies that of no other organ, the encephalon excepted.

When the lungs are inflamed, a phenomenon occurs, which is supposed by physiologists to take place in health; animal heat is generated in them, as in any other part suffering under the inflammatory process.

In inflammation of the brain, there is an apparent increase of energy; but this is merely the expenditure of vital force already existing, and its conversion into heat. Many months may elapse before the nervous power, thus lost under encephalitis, is recovered.

Thus, disease of the lungs, the blood, and the brain, the three principal links in the chain through which physical force is metamorphosed into vital, is productive of prostration to an extent far exceeding that which arises from the disorders of other parts of the system.

XLII. According to the views expressed in this paper, all the forces that act upon the organic being have an extrinsic origin. While light comes from the remotest regions of space, and other forces are derived from surrounding media, both solid and aeriform, the vital force which polarizes these, and thus harmonizes the entire nervous system to their influence, is also derived from without, at least in part, it being due to the union of the atmosphere with the blood.

It is to be hoped, for the honour of physiology, that the doctrine hitherto held, of the nervous centres generating their own vital force, and of the lungs being a seat of combustion, will be abandoned. The one would lead to mental imbecility in the space of a day, even if it were possible for an equivalent of vital power to be derived from the solution of nervous structure; while the other would conduce instantly to inflammation.

How much more consonant is it with the well-known economy of nature to assume that she would not submit her most delicate structure to the destructive process of generating electric force, by means of its own organic changes, when the contact of the atmosphere with the debris of the system, under chemical laws, is adequate to the purpose demanded: a plan so simple as to excite wonder, and, at the same moment, so expressive of nature's resources, who, out of the waste of the organization—the carbon of the lungs—resuscitates the system, maintaining its continuous vitality by the aid of that breath of life from without, of which the supply is inexhaustible.

Racine, *Wisconsin*, 1854.

ART. V.—*On the Climate and Salubrity of Fort Moultrie and Sullivan's Island, Charleston Harbour, S. C., with Incidental Remarks on the Yellow Fever of the City of Charleston.* By JOHN B. PORTER, M. D., Surgeon U. S. A. (Continued from p. 75.)

Etiology.—In this inquiry, it is proposed to include both Charleston and Sullivan's Island; for a comparison of the two places is desirable, in order to show that the type of disease on the island is assimilated to that of the city, which must be the case if the same causes are in action under similar circumstances. The generation of yellow fever appears not to depend on the presence of one particular cause, but on the presence or combination of several; and the following seem to have been the principal ones in 1852, as in previous years, viz:—

1. *Intemperance, or the excessive use of Alcoholic Liquors.*—We have sufficiently alluded to the pernicious effects of the free use of spirituous liquors on the system in hot climates. In all cities there is quite too much drunkenness, and Charleston is no exception to the remark, as the numerous licensed groggeries and their customers show, as well as each September election for municipal officers. Dr. Thomas Y. Simons, port physician of Charleston, uses the following language in relation to this election in 1852:—

"Early in September, the fever became epidemic, among the Irish and Germans particularly. It was during the excited elections for Mayor and Aldermen of Council, in which elections these two classes of our citizens are always brought actively into play, exposed to the influence of every species of exciting cause used under such circumstances, and who constitute infinitely the largest proportion of those who become victims of the fever.* * * So satisfied were the public authorities of the great influence of this cause, that they petitioned the Legislature to change the time of election of Mayor and Aldermen, which has been granted."

On Sullivan's Island, the chief source of revenue is from the sale of liquor-licences; and there were not less than thirteen licensed grogshops in 1852, in a population of perhaps two thousand in summer, and as many hundreds in winter, besides illicit venders, all of whom dispensed the vilest compounds to their unfortunate customers.

2. *Fatigue and Exposure.*—Dr. Carpenter says:—

"The fatigue resulting from excessive muscular exertion is commonly accounted one of the predisposing causes of zymotic disease; and this, too, is usually supposed to operate merely in occasioning a general depression of the vital powers. All muscular exertion, it is now universally admitted, involves as its condition a disintegration of muscular tissue, the components of which normally undergo oxidation, so as to be partly eliminated by the respiratory process under the form of carbonic acid and water, and partly by the kidneys under that of urea, &c. Now, if the disintegration of muscle by exercise take place faster than the matter thus set free to decompose can be oxidated and eliminated, it must remain in the blood for a time in that very state of readiness to change which renders it peculiarly fermentable; and thus its presence

in the circulating current will give to the blood the same susceptibility to the action of zymotic poisons which it will derive from any of the causes."

The number of strangers at hard labour in Charleston during the hot weather of summer is large, and it is this class which usually suffers at the commencement of all epidemics. "There were a great number of emigrants from Ireland," says Dr. T. Y. Simons in 1852, "who arrived here during the epidemic, and soon became the victims of disease. Destitute of means, they were compelled to do laborious work, such as excavating the earth, or paving the streets, &c., and having miserable accommodations."

On Sullivan's Island, the condition, habits, fatigue, exposure, &c. of the same class of people is similar. During the summer of 1852 the garrison of Fort Moultrie was small, and for a time the men had to go on guard every alternate day, the other day being devoted to severe fatigue duty, as getting wood and water, draining the fort and parade, which had been deluged by the heavy rains, &c.; so that their fatigue and exposure were very great. The company women (laundresses) also suffered from fatigue and exposure. These two classes of persons, the enlisted men and company women, chiefly suffered from yellow fever.

3. *Imperfect Ventilation.*—On this cause of disease, Dr. Carpenter's remarks are so interesting as to justify our giving a long extract:—

"An accumulation of disintegrating matter in the system may be due, not merely to its excessive production, but to any obstacle which interferes with its due elimination; and this will especially be the case when the respiratory process is imperfectly performed. All physiological and pathological evidence tends to indicate the paramount importance of this process, not merely as regards the direct elimination, through the lungs, of a large amount of matter which is undergoing change; but also as furnishing the conditions by which the matters properly to be excreted by the kidneys are brought into the normal condition for being thus eliminated. For any prolonged deficiency of respiration necessarily lowers the general oxidating process throughout the body; and thus it happens that an undue amount of carbonaceous matter is thrown upon the kidneys for excretion, and that the highly azotized compounds are not so completely brought, as they should be, into the condition of urea. Now, that over-crowding, and consequently deficient aeration of the blood, is one of the most frequent causes of the severity of epidemics, is a fact so universally recognized by all who have attended to the subject, that we need scarcely do more than advert to it. * * * The evil results of an insufficient supply of air are not exerted merely through the imperfect oxidation and elimination of the substances which are undergoing decomposition within the system; for the same cause will operate to confine the putrescent effluvia that are given off *as such* from the lungs and skin, which will produce the effect upon the individuals habitually exposed to them, as if these were generated from some external source. It was ascertained by the experiments of Collard de Martigny, that the fluid exhaled from the lungs is by no means pure water, but contains as much as three parts in 1,000 of organic matter. If this fluid be kept in a closed vessel, and be exposed to an elevated temperature, a very putrid odour is exhaled from it; and from the recent experiments of Mr. R. A. Smith, it appears that its putrescence depends on the decomposition of an albuminoid substance. There is every reason to believe that the fluid exhaled from the skin is charged with a very similar substance; its presence being indicated by the foul odour of garments that have been too long worn. And thus imperfect ventilation becomes the means not only of preventing the due elimination of decomposing matters from the body, but actually of reintroducing its poisonous

products into the blood, by the very process which was designed for the purification of the vital fluid."

Bad or imperfect ventilation, then, may be one of the main causes of an epidemic; and the city of Charleston is peculiarly obnoxious to this cause of disease, it being sufficient in this place to merely refer to the close and ill-ventilated tenements in all parts of the city—the narrow streets, and the dirty lanes and alleys, inhabited by gregarious foreigners and families, where almost every morbific cause is in full operation.

We will quote from the *Charleston Evening News* of April 29, 1853, in confirmation of the foregoing statement in regard to ventilation:—

"We have narrow lanes into which are crowded a larger number within given spaces than should occupy them. We lay out streets, indifferent, not only to the comfort, but the health of those who are to dwell in them, at all seasons, during the prevalence of heat that enervates and predisposes to disease, as well as the cold that excludes it. We extend our municipal limits, anxious to enlarge our taxable resources without providing those places of general resort and recreation, public squares, which have been aptly called the lungs of cities. In our engrossing anxiety not to leave land unimproved, even where land is in excess, we are providing the sources of disease and death for posterity."

"Here, then, are the latent causes of contagious and malignant disorders in our cities. They are built up without regard to due ventilation. When the existing streets and thoroughfares do not admit of full improvement, from a defective plan of original construction, in opening new streets the opportunity is lost of having them of proper width, and making spaces for squares and parks that will allow of a free circulation of air. This branch of municipal legislation should receive more attention than it has heretofore met with in this locality."

On Sullivan's Island this cause of disease is not so powerful as in Charleston. The dwellings are further apart on the island than in the city, and there is a better circulation of air; but still, there is imperfect ventilation in the crowded old houses and kitchens, the floors of which are on the ground, and their timbers decayed. These decaying houses are the most liable to be crowded; and one can be pointed out which, in 1852, contained three Irish families, a free negro or two, and in the house and yard there were cats, dogs, pigs, cows, chickens, &c. The whole house was not comfortable, or decent, for one small family. In the fort the ventilation is, in general, good; best in the officers' quarters, next in the men's barrack rooms, and worst in the servants' rooms, which are on the ground-floor. The barrack rooms are on the same floor with the officers' quarters, in the second story, and are much more airy, though more crowded, than the servants' rooms. Every man had a bunk for himself. The companies being small, the rooms were not so much crowded as might be supposed; but, when compared with the officers' quarters, there was imperfect ventilation. The men, for this reason, have a practice of sleeping on the piazza in warm climates; a practice which is unhealthy, and, in the fever season, dangerous.

4. *High Solar Heat.*—This acts in two ways; on the human system, and by rapidly promoting animal and vegetable putrefaction. It is always necessary for the production of yellow fever, and always prevails in the har-

bour of Charleston. From the tables of Dr. J. L. Dawson, City Register, it appears that the mean summer heat in Charleston is great, and the Meteorological Registers of Fort Moultrie show that it is uniformly so on Sullivan's Island.

Abstract of the Detached Thermometer at Fort Moultrie for Five Years.
Average mean temperature.

MONTH.	Sunrise.	9 A. M.	3 P. M.	9 P. M.	Daily mean.	Highest degree.	Lowest degree.	Range.	Hottest daily mean.	Coldest daily mean.	Range.
1849.											
May	69.48	74.13	75.25	71.71	72.37	84	54	30	79	61	18
June	76.70	80.40	81.86	78.33	79.13	92	74	18	86	74	12
July	76.51	79.87	81.29	78.58	78.87	88	68	20	85	69	16
August	78.35	82.80	83.58	80.58	80.80	91	73	18	85	75	10
September	72.16	75.83	78.16	75.03	75.16	86	64	22	81	69	12
1850.											
May	66.38	73.61	76.83	70.22	71.61	84	56	28	78.0	61.5	16.5
June	71.00	79.66	81.46	75.23	76.23	93	55	38	83.0	63.0	20.0
July	79.03	85.70	88.19	82.12	83.61	98	74	24	90.0	78.5	11.5
August	79.00	85.00	87.29	81.93	83.14	94	73	21	87.5	77.5	10.0
September	73.10	78.90	82.76	77.50	77.93	91	60	31	84.0	68.0	16.0
1851.											
May	67.64	76.38	77.77	72.00	72.70	91	53	38	83.5	58.5	25.0
June	74.56	80.80	82.53	77.90	78.55	98	60	38	84.5	65.5	19.0
July	78.32	83.32	88.67	81.03	83.50	100	70	30	90.5	76.0	14.5
August	78.29	83.16	86.03	81.19	82.16	92	66	26	86.5	74.0	12.5
September	68.06	76.00	79.46	73.76	73.76	91	52	39	83.5	62.0	21.5
1852.											
May	67.74	77.22	78.12	72.12	72.93	93	49	44	81.5	57.0	24.5
June	71.13	78.50	80.93	75.90	76.03	99	63	36	86.5	68.0	18.5
July	76.29	83.41	85.35	80.54	80.82	92	72	20	85.5	76.5	9.5
August	74.38	81.87	84.03	78.87	79.20	95	66	29	85.5	68.0	17.5
September	70.50	77.83	79.00	75.70	74.75	92	61	31	81.0	67.5	13.5
1853.											
May	68.32	75.96	76.77	72.54	72.54	84	56	28	78.0	63.0	15.0
June	74.10	82.73	82.80	77.93	78.48	96	66	30	84.5	72.5	12.0
July	79.29	84.87	86.41	80.74	82.85	91	72	19	86.5	76.0	10.5
August	75.87	83.51	84.29	79.74	80.08	95	66	29	85.0	71.0	14.0
September	72.36	79.06	81.30	75.50	76.83	95	57	38	84.5	64.0	20.5

These tables show a uniformly high summer temperature, but not in the same degree, for the unhealthy summer of 1852 was not so hot as the healthy one of 1853; 1849 was the coolest, and 1850 and 1851 were the warmest. But it is well known that high heat alone, though indispensable, does not produce yellow fever. It is a powerful predisponent, and develops other active agents.

The summer temperature of Sullivan's Island is high; and when the seabreeze dies away, as is often the case at ebb tide, or there is a west wind, the heat is oppressive. There is not a hotter street in the United States than Middle Street. The surface of the island is composed of sand; but little

grass grows to absorb or soften the sun's rays; there are few trees, and none for shade; the houses are retired from the street, so as to afford no protection from the sun; and the direct and reflected heat is often intolerable. Persons whose pursuits allow them to remain within doors during the day, are pretty well off; but the active man of business will find that Charleston is more tolerable than Sullivan's Island, from sunrise to 11 o'clock A. M., and from 3 P. M. to sunset, the shade of the houses affording great protection from the sun. The great advantage of an island residence is in the nights, which are not so uncomfortable as in the city.

5. *High Dew-Point.*—With a high altitude of the thermometer in the low country of the South, humidity and a high dew-point are always associated. This is the case in Charleston Harbour. Dr. Ramsay says: "The hygrometer in Charleston shows an almost constant humidity in the air." Dr. William Hume, of Charleston, says: "We live in a climate with a dew-point above 70°; the baneful sirocco frequently prevails during the day, while the night is *murky*, likening our city to another place, which Shakspeare speaks of, on the high authority of one of his witches." The meteorological observations of Dr. J. L. Dawson, City Register, for two years, and those at Fort Moultrie for three years, in relation to the dew-point, are given below.

Mean.

MONTH.	FORT MOULTRIE.			CHARLESTON.	
	1849.	1852.	1853.	1852.	1853.
May	70.19	70.51	66.50	64.35	64.70
June	75.36	72.94	71.40	66.63	70.43
July	76.27	78.70	77.33	73.16	75.64
August	77.69	75.77	75.32	70.70	74.45
September	71.70	71.74	71.85	67.66	70.70

Humidity (a high dew-point) plays an important part in the causation of febrile diseases. Speaking of the various causes which have been considered as producing yellow fever in Charleston, Dr. Thomas Y. Simons, Port Physician (*Charleston Medical Journal* for May, 1853), says:—

"Altitude of temperature is indispensably necessary. Moisture is undoubtedly a very important agent, and the dew-point indicates a moist atmosphere during its prevalence. Hence the importance of thorough draining and paving to prevent much evaporation."

There can be no question but a high dew-point is a powerful agent in the causation of fevers; we may go further, and affirm that yellow fever never prevails in a place, endemically or epidemically, unless there is a high dew-point. But we have no idea that this is the sole cause of the disease, for in Charleston Harbour it is always high in summer, and the fever occurs only occasionally, there having been long intervals of health, as from 1839 to 1849. Indeed, heat, humidity, and a high dew-point are always present in summer, as is evident to the most common observer, in the rapidity

with which butcher's meat takes on the putrefactive process; yet we do not meet with the yellow fever annually. It is the concurrence of heat, moisture, and a high dew-point which produces the weather termed *close, sultry*, or *muggy*, and which also produces the change of complexion, the languor of mind, and the debility of the muscular system; and it is the change from this state of things—from high and continued heat, excessive and long-continued humidity, and a continued high dew-point, which renders a visit to the North, or to any dry, elevated section of country, in summer, so agreeable, and so conducive to health, producing a change in the colour of the skin, energy of mind, and strength of the muscular system.

Long-continued heat, humidity, and a high dew-point, are the characteristics of the summer climate of the South, in contradistinction to that of the North. The heat and humidity at the North are often great and the dew-point high, but they are of short duration; and it seems a little singular that people should forget the importance of a high dew-point; hence probably, the notices which appeared in the Charleston newspapers, in the summer of 1853, in praise of their city summer temperature, and comparing it with the northern cities, as Boston and New York, at the same time, and entirely overlooking the effect of a high dew-point. Give us a low dew-point and it matters not what is the altitude of the thermometer. In 1853, the weather was extremely hot in all the northern cities—in New York, for instance—but it was for only a few days in succession, for thunder and hail-storms suddenly put an end to the hot weather, at least for a time. Here is the essential difference between a northern and a southern climate.

The great humidity of Sullivan's Island becomes evident to all by the rapid oxydation of all articles of cutlery, the rusting of keys in one's pocket, the mildew on linen clothing, the injury done to cloth garments, the mould on boots, shoes, harness, and all articles made of leather, &c. Common table salt is an excellent practical hygrometer, and every good housewife knows how frequently it is necessary to submit this essential article of subsistence to a drying process. A familiar article of convenience, matches, affords another example.

In Dr. Simons's report of 1839, there is the following passage:—

"Our meteorological observations give us but little light on this subject; but from my own observations, I have remarked that the dews are heavy and the air chilly at night, and that cloths and leather become mouldy. Brasses can hardly be kept clean, and steel quickly rusts. The continued solar influence in the surrounding water, causes an immense evaporation, which ascends high, and, after the sun declines, begins to descend in the form of dew, in which, I believe, malaria floats."

High summer heat, humidity, and a high dew-point are always found on Sullivan's Island, in as marked a degree as in Charleston; but the officers' quarters and men's barrack-rooms at the fort are as comfortable and as free from dampness as any rooms on the island, not being in casemates, and in the second story; the lower story, or ground-floor, however, is very damp, and

wholly unfit for residences; and the brick pavement[1] in front is always damp and covered with fungi. This story was but little occupied in 1852, principally by a few servants and the guard, the guard-room being on a level with the ground-floor of the barracks.

In consequence of the combination of heat and moisture, the island is infested with insects, as fleas, sand-flies, house-flies, mosquitos, ants, cock-roaches, &c.

6. *Defective Drainage.*—This cause of disease is common to both Charleston and Sullivan's Island. In the former, many persons are aware of the necessity for introducing a new and better system of drainage. We may refer for the proof of this fact to an editorial article in the *Charleston Mercury*, for October 1, 1852, and the *Charleston Evening News*, for November 9, 1852; and to a paper by Dr. Hume in the *Charleston Medical Journal*, for January, 1850, and January, 1853; and another by Dr. Thos. Y. Simons, in the same Journal for November, 1851.

After the heavy rains in 1852, we saw men baling water out of cellars with buckets, in Church Street, which is several feet above the level of the wharves on East Bay. A physician who resides above the level of Church Street, only a few doors from Meeting Street, which is one of the best streets in Charleston, told me that it took two men eight hours to bale the water from his cellar in September, 1852; and, said he: "After all, the dampness remained." In the summer of 1853, the city authorities authorized and enjoined the employment of the city engineers, to free cellars from water, but "after all, the dampness remained." Water is so constantly found near the surface, that cellars ought not to be allowed; and in truth, a cellar ought never to have been constructed in the city of Charleston.

There is no drainage on Sullivan's Island, except the most trifling surface drainage. The whole island is composed of sand, which is very porous, and water is found just below the surface in the driest season, this surface water being always a mixture of fresh and salt water, which is insalubrious. The tides wash up sand, mud, and slime, particularly on the back beach, which is exposed to the intense heat of the sun at the ebb, and permanently as the spring-tides gradually subside to neap-tide. Both Pringle and Rush refer to this state of things.

Sir John Pringle's description of Brabant suits Sullivan's Island exactly. The water in the wells is always, even in the driest summer, near the surface. There is a "Potter's-field" on the east end of the island, in a part termed the "Myrtles," and here the graves have to be baled out in the driest season just before interment; and, in a wet season, the water runs into the grave faster than it can be baled out, so that a weight has to be attached to the coffin. We now see prominent reasons for the production of malaria (for want of a better term) in the driest summer, as in that of 1850, when

[1] Brick is unfit for floors unless previously dipped in boiling pitch or asphaltum.

the intense heat, humidity, and high dew-point evolved it with rapidity, and there was epidemic fever.

Dr. Ferguson, of the British Army, a physician of great experience, says that vegetation is not necessary to produce malaria, and that this poison may abound where there is no vegetable matter to decay. He says:—

"In August, 1794, after a very hot and dry summer, our army in Holland encamped at Rosendaal and Oosterhont. The soil, in both places, was a level plain of sand, with perfectly dry surface, where no vegetation existed, or could exist, but stunted heath plants. It was universally percolated to within a few inches of the surface, with water which, so far from being putrid, was perfectly potable. Here fevers of the intermittent and remittent type appeared among the troops in great abundance. It is interesting to observe that the soil in Walcheren is precisely similar. Sir Gilbert Blane describes it as consisting of a fine white sand, known in the eastern counties of England by the name of silt, and about a third part of clay. It was after a hot and dry summer, also, that the British army suffered in that island from the endemic fever, to a degree unprecedented in the annals of warfare."

Dr. Watson (Lectures) says:—

"Facts like these seem to prove that the malaria, and the product of vegetable decomposition, are two distinct things. They are often in company with each other, but they have no necessary connection. Whoever, in a malarious country, waits for the evidence of putrefaction, will wait, says Dr. Ferguson, too long. For producing malaria, it appears to be requisite that there should be a surface capable of absorbing moisture, and that this surface should be flooded and soaked with water, and then dried; and the higher the temperature, and the quicker the drying process, the more plentiful and the more virulent (more virulent, probably, because more plentiful) is the poison that is evolved."

* * * * * *

"No very certain or extensive observations have yet been made in respect to the kind of soil from which the miasmata are most apt to be extricated. That which is loose, penetrable, porous, and sandy, appears highly favourable to their formation. So are soils which, containing much clay, are very retentive of moisture."

Sir John Pringle says:—

"And here it may be proper to distinguish between the moist and the rainy seasons; for, in marshy grounds, intense and continued heats, even without rain, occasion the greatest moisture, by the exhalation which they raise and support in the atmosphere; whereas frequent showers, during the hot season, cool the air, check the rise of the vapours, dilute and refresh the corrupted water, and precipitate the putrid and noxious effluvia. But if heavy rains fall in the beginning of summer, and are followed by great and uninterrupted heats, then the water collected by the rain, stagnating in the lower grounds and corrupting there, furnishes matter for more exhalation, and thereby renders the season more sickly, and the diseases more fatal."

"It ought also to be remarked, that the sickness never begins till the heats have continued long enough to give time for the putrefaction and evaporation of the water."

Dr. Rush remarks on this passage:—

"This has too generally escaped notice; hence we confound a moist with a rainy season. A wind often brings a moisture with it, which discovers itself in producing more obvious effects upon metals, and papers, and even wood, than a heavy rain. It likewise disposes the miasmata which produce bilious fevers to be more general, and more active in their effects upon the body."

The foregoing extracts from the writings of Sir John Pringle and Dr. Rush are as applicable to Sullivan's Island as if they had been penned for an exact description. The time of the heavy rains has an immense influence on the health of residents, for it may happen that an intensely hot and very dry summer, the sun acting on the previous heavy autumn and winter rains through the porous and sandy soil, may be unhealthy, as in 1850; and on the other hand, a wet summer may be very sickly, like that of 1852, when the excessive rains, combined with the heat, act both as predisposing and exciting causes of fever.

Can Sullivan's Island be drained? The solution of this question is important. If the island cannot be drained, severe epidemics must occur, as they have always occurred, at intervals. Certain lots may be filled with sand, or superficially drained; but more than this is required, especially on the back beach or north side of the island. A thorough system of deep or subsoil drainage over every part of the island is necessary, at least from the hotel (Moultrie House) on the east to the steam ferry on the west; and, as the settlement extends, the drains should also be extended. The faster the island fills up with inhabitants, the more essential are drainage and every part of sanitary police; for the more the island village assimilates to a city, the more necessary will sanitary measures become, and still more will the diseases resemble those of Charleston than hitherto. That the island can be drained in the manner indicated, there is no doubt. General Totten, chief-engineer of the army, assured me in a conversation last spring (1853), that the measure is practicable. In our humble opinion, nothing will more conduce to salubrity, or more surely prevent periodical visitations of epidemic disease, than a well-regulated system of drainage.

7. *Bad Sanitary Police.*—This is an extensive subject, embracing almost every point of city mismanagement. Under this head may be included animal and vegetable decomposition; and, in strictness, bad ventilation and bad drainage also. It is a disagreeable subject, for the residents of a place dislike to be told that their streets, lots, houses, &c. are in bad condition and filthy; nevertheless, *magna est veritas et prævalebit.* Owing to the natural difficulties which existed at the foundation of the city in 1680, Charleston has always been in a bad state of sanitary police, including both bad drainage and bad ventilation; and this was unavoidable at an early period of her history, and for several years after the settlement of the town. The city seems to have arisen without any plan, in those days when "every man did that which was right in his own eyes." The consequence is, that the streets are irregular, narrow, and badly ventilated; and, though favourably situated for commercial purposes, nature has interposed great obstacles to the growth of a healthy commercial city. Dr. Thomas Y. Simons says:—

"From the old maps it is evident that Charleston was intersected by marshes and creeks, which are now filled, and houses built thereon. At present, the city is surrounded by marshes, through which the salt water ebbs and flows."

Dr. Ramsay, in his *History of South Carolina*, states:—

"George Chalmers, in his *Political Annals of the United Colonies*, printed in 1780, pp. 541, 542, observes that 'Charleston was long unhealthful. From the month of June to October, the courts of justice were commonly shut up. No public business was transacted. Men fled from it as from a pestilence, and orders were given to inquire for situations more friendly to health.' This statement is corroborated by tradition from the elder citizens, who inform us that in the time of their fathers, the sick were sent from Charleston to expedite their recovery in the more wholesome air of the country; and that the country was preferred, on the score of health, as a summer residence. This is by no means improbable. The situation of Charleston in its natural state, was a slip of land stretching southerly between the two rivers, and projecting into the harbour formed by their junction, and divided into a number of peninsulas by creeks and marshes indenting it on three sides, so as to leave but little unbroken high land in the middle. The first buildings extended along East Bay Street, and had a marsh in their whole front. A considerable creek, named Vanderhorst's Creek, occupied the foundation of Water Street, and passing beyond Meeting Street, sent out a branch to the northward, nearly to the Presbyterian church. Another creek stretched northwestwardly nearly parallel to East Bay, from the neighbourhood of Macleod's Lots, through Longitude Lane, and to the north of it. The same kind of low ground ran up Queen Street, then called Dock Street, beyond the French church and through Berresford's Alley, till it approached Meeting Street. The north end of Union Street[1] was planted with rice about the middle of the eighteenth century. Another very large creek occupied the site of the present central market, and extended westwardly beyond Meeting Street, which diverged southwardly almost to the Independent church, and northwardly spreading extensively and dividing into two branches, running to the northwest and to the northeast so as to cover a large portion of ground. Besides the marsh and those creeks, which nearly environed three sides of the improved part of Charleston, there was another creek a little to the southward of what is now Water Street, which stretched westwardly over Church Street; and another, which ran northwardly up Meeting Street, and then extended across westwardly nearly to King Street. A creek ran from the west near where Peter Smith's house now stands, and nearly parallel to South Bay, till it approached the last-mentioned creek, and was divided from it by King Street and a slip of land on each side. Six other creeks ran eastwardly from Ashley River, three of which stretched across the peninsula so as to approximate to King Street. There were also ponds and low grounds in different parts of the town. One of these extended on the east side of King Street almost the whole distance between Broad and Tradd streets. This was granted to the French church in 1701, but being useless in its then state, was leased out by them for fifty years. In the course of that period the tenants improved and built upon it. There was also a large body of low grounds at the intersection of Hasell and Meeting Streets. The elder inhabitants often mention a large pond where the Court-House now stands.[2] It is believed that this, though real, was artificial. It is probable that the entrenchments attached to the western fortifications of Charleston, which extended up and down Meeting Street, from the vicinity of the Independent church to the vicinity of the Presbyterian church, were dug so deep as to cause a constant large collection of water at that middle part of the lines. It was the site of Johnson's covered half moon, and of a drawbridge, over which was the chief communication be-

[1] Union Street is now State Street. The street on each side of the central market, spoken of by Dr. Ramsay, is now Market Street.

[2] I have understood from gentlemen now living, that it is not much more than twenty years since ponds of water stood at the junction of Broad and Meeting Streets, in a rainy season, by St. Michael's Church and vicinity, extending up and down the latter street both above and below Broad Street, which ponds became stagnant, green, and at last dried up by the heat of the sun. This is the condition of several localities, in mid-summer, at the present day.

tween the town and the country. No prudent engineer would erect such works as these on a pond, though, when they were erected on the soft soil of Charleston, they would be very likely to produce one. Whether this was a natural or artificial collection of water, there was enough in other parts of the town to make it unhealthy. Such, with some alteration, was the situation of Charleston for the first seventy years after its settlement."

"To reduce such a quagmire as a great part of Charleston originally was, to a firm, high, and dry state, required time, labour, and expense. Much has been done, but much remains for future enterprise."[1]

We have given this long and instructive topographical description from Dr. Ramsay's History, in order to show that there have been abundant local causes of fever, from the foundation of the city to the first part of the present century; and we will proceed to show, if we have not already shown, from other writers, that extensive local causes of disease have existed from Dr. Ramsay's time to the present day.

Dr. Thomas Y. Simons says:—

"Gradually and progressively, all of the ponds and creeks were filled up. Subterraneous drains in the different streets were made, and drains from the yards of the respective premises were made to communicate with the drains in the streets. But it was found that the drains having the grates in the centre of the street, and becoming thus filled up, caused, after heavy rains, a vast quantity of water to be accumulated in the streets, making them at times almost impassable. The offals of the city were required regularly every day, except Sunday, to be removed."[2]

"The city of Charleston was originally intersected with creeks, and a quantity of marsh in the midst. These creeks and marshes were filled up with wood, and the offals of the city, composed of an immense mass of corruptible matter; a deleterious miasma or vapour is constantly arising from the surface, from this cause, and the accumulated filth which daily occurs."[3]

What must be the effect of digging and cleansing drains, laying gas-pipes, and making other excavations, particularly about the wharves, throwing up in midsummer, as in 1849 and 1852, this "corruptible matter," decaying wood, city offal, &c.? Would stringent quarantine have excluded the *materies morbi?*

Dr. Ramsay again:—

"The original settlers of Carolina had no thought that in less than a century Oyster-Point would become a place of commercial importance, and the capital of an independent State stretching from the ocean to the mountains. Had they anticipated half of what has already taken place, ten feet alleys, and streets thirty-three feet wide, would have made no part of their projected seat of government. It would then have been nearly as easy to have made the streets one hundred feet wide as any inferior number. In that case they would have admitted three rows of trees, one at each side, and one in the middle of every street. It would have been easy to have made no lots of less size than half an acre, and by law to have prevented their subdivision. In addition to the inconveniences of a low and moist situation, too many people in Charleston, in consequence of its niggardly plan, are crowded on too small a space of ground. Close, compact cities are the destroyers of the human race.[4] Every family

[1] History of South Carolina, vol. ii. pp. 70–73.
[2] Charleston Med. Journ. November, 1851, p. 794.
[3] On the Yellow Fever of 1824.
[4] Recent investigations fully confirm the assertion of Dr. Ramsay, particularly the reports of the poor-law commissioners on the sanitary condition of the labouring population of Great Britain.

generates a portion of filth, and when they are near to each other, that becomes too great for the health of the citizens. Numbers are every year sacrificed to the avarice of the proprietors of lots. The evils of a crowded population are increased by high and close fences, which are daily increasing, and still more by building houses in contact with each other and without any interstices between. The daily removal of putrescible substances lessens the evils of an impure air, but is inadequate to the purpose intended. The only effectual remedy is fresh running water. This unites cleanliness with coolness. It removes noxious vapours, cools the atmosphere, and increases its salubrity by extricating fresh and wholesome air from its own substance. The next best practicable mitigator of heat and corrector of foul air in Charleston is trees planted in all the streets which can admit them. They are the coolers given to us by Nature. In addition to their refreshing shade, they imbibe the poisonous materials which vitiate the air. They fan the earth by the vibratory motion of their leaves. Instead of obstructing the free circulation of the air, they increase a light breeze by creating an undercurrent on the surface of the earth, where it is wanted. Cities built with marble, if destitute of trees and vegetation, would only afford a miserable residence to splendidly wretched inhabitants."[1]

The foregoing extract from Dr. Ramsay is long, but every line is full of instruction. The subject of wide streets, trees, bad ventilation, filth, and all the evils of a crowded population, and the fresh running water, are lessons to city legislators in all future time. One might conclude that the historian presaged the erection of the New York Croton Water-Works, and a similar structure for Charleston. The passage describes the past, the present, and the future.

Dr. Shecut, who wrote in 1819, describing the yellow fever of 1817, says of Charleston, at that period:—

"Among the multiplied causes of infection in cities, it must be admitted that the innumerable sinks and drains, if not the primary, are assuredly the secondary causes, and qualify the atmosphere to support for a longer time than it otherwise would, the stamina of the infection, by communicating almost hourly fresh supplies of their effluvia, and that this is the case may be readily perceived, when we take notice that the infection, which is productive of the peculiar disease of the city, does not extend its influence to more than half a mile without its limits. The drains, it must be acknowledged, were sunk through the city, to prevent the accumulation of moisture, and other exciting causes of disease, and as long as they are kept clean, answer a very valuable purpose. That they are otherwise than clean, especially during the sickly season, is not to be attributed to the neglect of our police, who are indefatigable in their exertions, and in the performance of their duties; but to those thoughtless and lazy individuals, who wink at the duplicity of their servants and others, that are in the filthy habit of secretly depositing the offals of fish, fowl, and the smaller animals, into the drains, and sometimes into their sinks and sewers.

"I am perfectly satisfied that on this subject I am not alone; many citizens, particularly those that have been observant of the state of the city at different seasons of the year, have frequently, and especially in the latter parts of the evening, on passing certain drains, been almost overwhelmed with the stench that has issued forth from some of the grates; this circumstance has frequently occurred in particular sections of the city, particularly from Church Street to the bay, with the intersecting alleys; and I am warranted in saying that, in nine cases out of ten, the prevailing fever has originated within the limits of that section of the city.

"Owners of wharves and lots of low ground in the city, should be prohibited

[1] Hist. of South Carolina, vol. ii. pp. 101–103.

from raising mud to fill up their wharves during any of the summer months; but more especially those persons who are in the habit of filling up low lots with filth and rubbish collected in the yards and streets of the city. It is also a matter of notoriety, that the scavengers' carts convey, daily, to particular low spots in the city, but more generally the skirts thereof, promiscuous heaps of dead animal and vegetable matters, which, being deposited in heaps, become sources of infection, and greatly add to the general exciting cause of disease.

"In directing the attention of the police to the importance of filling up all low lands within the limits of the city, it is obvious that dry and wholesome materials are intended, and not the rakings and scrapings of the streets and yards. I would suggest the immense advantage of encouraging vessels to bring in dry sand and gravel as ballast, which might be purchased for the purpose of filling up all those situations, heretofore the sources of *infection*, near the bay."

We have, in Dr. Shecut's description of the city police in 1817 and 1819, in both of which years yellow fever prevailed, the condition of the streets, sinks, drains, yards, cellars, and warehouses; the deleterious effects produced by putrid vegetables, so applicable to Market Street in all summers; and the state of the wharves and low lots, filled up with mud and city offal in the summer months, showing their injurious effects in precisely the same way as did the filth and excavations in 1849 and 1852.

Dr. Shecut graphically describes Charleston, at the present day, from 1849 to 1852. The state of the drains, the alleys, the "putrid cabbages" of Market Street, the raising of mud to fill wharves, the filling up of low lots, with filth and rubbish, and the neglect of lots, houses, sinks, and cellars.

In 1825, Dr. Thomas Y. Simons, in relation to the yellow fever of 1824 says: "The city of Charleston was originally intersected with creeks, and a quantity of marsh in the midst of it. These creeks and marshes were filled up with wood, and the offals of the city, composed of an immense mass of corruptible matter; a deleterious miasma or vapour is constantly rising from the surface, from this cause, and the accumulated filth which daily occurs."

In 1839, Dr. Simons says (Report on Yellow Fever): "Our wharves are formed by the palmetto logs, and filled up with wood, stone, and earth. In the docks, a great deal of mud, with decomposed vegetable and other materials are thrown up by the tide, and at low water the exhalations are offensive; independent of this, in some wharves the drains pour out their contents."—P. 18. Again: "Although the marsh miasma cannot be considered as the essential cause, yet it cannot be doubted, where the tide is but partially flowed, intermingling salt water with fresh, and becoming stagnant, a very noxious exhalation, injurious to health, must result. There is, besides, another evil. Small houses are erected on some of these tide-lots, which are but partially filled up, and then oftentimes with heterogeneous materials, is not only a serious nuisance, but must necessarily be deleterious to health. Another circumstance, I may here mention, is worthy of consideration, viz., the disposal of the offal of our city. It is obvious that if the law requires the citizens to remove all the rubbish and offals from their residences, regarding them as nuisances, the concentrating them upon one spot, for the purpose of filling up streets made on our marsh lots, must be infinitely more pernicious and injurious, as well as improper."—P. 21.

The causes of disease enumerated by Dr. Simons seem to be amply sufficient to produce the yellow fever of 1838 and 1839, especially when it is recollected that the great fire occurred in April of the first year, laying bare a large part of the city, and exposing the cellars, sinks, &c., to the direct action of the sun.

In the *Charleston Medical Journal* for January, 1850, immediately after

the epidemic fever of 1849, and, in reference to it, Dr. Hume gives the "Causes of Yellow Fever in Charleston, with an Outline of a Plan for its Prevention," from which some extracts will be taken. Dr. Hume says:—

"Without entering into the details of removing the excess of organic matter and moisture from our soil, it will be obvious that the indications for preventing yellow fever are to reduce the soil to its pristine sterility and aridity, and thus break up the connection between the materials and the agents of putrefaction. How far this is possible, I leave to the intelligence of those in authority. That our present system is ineffectual, no one will deny. That a change is desirable, all will acknowledge. But fatalism, and a fear of unrewarded expenditure, deter us from the attempt to remove from our city those causes of disease which we have introduced. Accumulated filth of a dense population, in a warm latitude, has been a long-acknowledged cause of disease; and if that filth is not obvious to the sight, its existence is easily demonstrated by experiment; and its necessary presence must be acknowledged, on a little reflection. The sandy and porous soil on which our city is located, is well calculated to conceal the organic matter which is daily absorbed and retained until eliminated again, by solar heat, in the unwholesome and poisonous gases and vapours of the putrefactive fermentation. Should the daily contents of the Charleston market be scattered in our streets, and then allowed to putrefy, who would express astonishment at the prevalence of yellow, or any other kind of fever? Who would accuse our merciful Father of having inevitably doomed our city to fatal epidemics? Who would remain under this blind fatalism, and suffer an evil so obviously the result of our own production? Yet this is no exaggerated state of things. The contents of the Charleston markets are not thrown on the surface of the city, and then left to putrefy; but they are thrown into the interior of the earth, after being cooked and digested, mixed with the subterranean water, transmitted to every portion of the city, defiling the wells, and poisoning the atmosphere by the result of its ultimate decomposition. Is this true picture of the condition of our city less revolting, or less capable of producing disease, than the imaginary one of scattering our food on the surface? Yet, as the matter is concealed from our eyes, we do not appreciate the full extent of the evil. What we do not see, we have not conceived; but that a worse state than is generally supposed (if any one ever had any suppositions on so disagreeable a subject) does really exist, we have only to refer to the statistics of Dr. Lining, and we shall find that upwards of thirteen hundred tons of fecal matter are annually deposited by the inhabitants, with ten thousand tons of urine. Now, if we double those quantities, for the production of the lower animals, we shall have a comprehensive idea of the quantity of organic matter which is annually submitted to the putrefactive fermentation, in our clean city of Charleston, with its population of thirty thousand.[1] Shall we omit to add to this immense mass of corruption, the two hundred dead bodies which are annually deposited in our graveyards, for fear that the truth may outstrip the imagination, and incredulity be awakened to deny the whole computation. Be it so; to my mind, it is a far greater wonder that we ever escape its influence, than that we are occasionally subject to its visitations. And I feel infinite gratitude to the Almighty dispenser that we are occasionally saved from that affliction which our ignorance and negligence have so justly merited."—Pp. 29, 30.

The views contained in the foregoing extracts from Dr. Hume command our hearty concurrence. We see the causes of yellow fever in Charleston, particularly in 1849. Very much of the article applies to Sullivan's Island, as filth, moisture, heat, the sandy and porous soil on which privies are situated, &c.

[1] Since 1849, Charleston Neck, containing about 15,000 inhabitants, has been annexed to the city proper, so that Charleston now has a population not far from 45,000.

We pass to the last year in which yellow fever prevailed in Charleston and on Sullivan's Island, and we find, in the *Charleston Medical Journal* for January, 1853, that Dr. Hume again treats of the "Causes of Yellow Fever in Charleston," having special reference to the epidemic of 1852, in which he uses the following language:—

"Cool nights, warm days, and a high dew-point, are the meteorological characteristics of a yellow fever summer in Charleston. Warm nights, warm or cool days, and a low dew-point, are the concomitants of a non-yellow fever summer."—P. 58.

"Influenced by this conclusion, that the wetness of the earth was the cause of the yellow fever, I published an article, recommending a system of underdrainage; and to obviate the then condition of the earth, I advised all citizens to pump the excess of water out of their wells into the city drains, so as to sink the water in the earth to a wholesome depth, such as I observed in the healthy summer of 1826. That nothing was done, I need not say, or that the fever did not abandon our city."—P. 60.

Again, p. 65: "If the result of the examinations of the last eight years shows that the yellow fever only occurs when the nightly condensations are abundant, and the materials of vegetable and animal origin in the earth are fully supplied with water to aid and hasten their decomposition, and, of course, eliminate in greater quantity and greater intensity the secondary products of putrefactive fermentation, which we have called malaria, for want of a better term, and limit it to the surface of the earth, instead of diffusing it into the higher atmospheric regions, beyond the reach of our respiratory organs, or the contact of the skin, it is clear that we have a combination of two meteorological conditions, the concurrence of which is necessary to produce that condition of the air which is inimical to human health. Our examinations also show that either condition, which I have termed malarial (which implies both the presence of the fermenting mass and the condensation after evaporation), or pluvial (which simply implies rain, or sources of water from any other cause occasionally), may exist, and no yellow fever be engendered; this separate harmlessness suggests at once to us the remedy that must be applied to prevent the deadly combination."

We will refer to other authority on the subject of sanitary police.

Charleston Evening News, Oct. 30, 1852: "*Health of the city.*—There is no fact better established in hygienic history, than the danger of exposing, in warm latitudes, a new surface to solar heat. So deleterious is it to health in rural districts, that even the digging of ground for gardens is avoided in the hot season, and wherever mill-dams or other excavations are made, during such seasons, the upturned soil has been found to contain the seeds of death in the form of malaria."

Again. "But not only have we uncovered to the action of heat a large surface of new soil, but added to it material dug out of the earth, where it has lain imprisoned for nearly half a century, and spread it over the depressions that we cannot elevate fast enough for unhealthiness. We have brought out of the foundations of the new custom-house, in a season the most favourable, from heat and moisture, to the development of malaria, the fetid and putrefied masses of earth which contain the elements of death, and scatter them in cart-loads over certain sections of the city.

"In dwelling on the circumstances of the excavations for the foundations of the custom-house, we are not unmindful of those made, in the upper portions of the city, for drains and gas-pipes, as among the causes which have occurred in producing a vitiated condition of the atmosphere. But we believe that the excavations for the custom-house have been of the greatest degree of injurious influence, from the character of the material dug up, and its being spread over a large extent of surface. Can we look for continued health in the city of Charleston, from proceedings which are attended with malady everywhere else? Can

we expect any other result than disease, when the atmosphere is prepared for the reception of morbid matter, from the degree of heat with which it is pervaded, and the quantity of moisture with which it is loaded? In such an atmospherical condition, if there were no other to give fatal activity, prepared as it is to receive malarious material, we are in our midst preparing a laboratory of poison, by unlocking the earth of the decaying substances so long imbedded in it, and spreading them over as large a surface as possible, where the sun and wind do the rest."

* * * * * *

"Our streets and lots have been formed of late, to a greater extent than ever before, of this material. Acres on acres of land have been made not only of upturned soil in the western part of the city, but of scrapings of kitchens, the sweepings of yards and gardens, with the blended vegetable and animal matters which have come to the surface from excavations. As then there can be no fact more strongly impressed, as a lesson, on the administration of Health Police, than that disturbance and removal of such matter in summer is injurious to the health of cities, and particularly that excavations where land has been made by filling up with wood, which has undergone decay, is sure to be followed, in a certain state of the atmosphere, by malarious emanations, all such operations should cease at the commencement of the hot season, say the 1st of May, and whatever material remains capable of forming a ferment, should be covered over with some disinfecting substance.

"Do not our numerous fires, followed by unhealthy seasons, afford an instructive analogy? We have scarcely ever had a large fire unattended by such a season, just previous to, or during warm weather, and the atmosphere was prepared to receive the miasm that would thence be formed. The principle of mischief here, is the same as emanations from putrescent materials collected on or below the surface. These are the ingredients in all such cases alike of that material of miasm, which is destructive of health from animal and vegetable decomposition. The new surface exposed in case of large fires, is enough to poison the sources of life, under a due degree of heat and humidity. Let us look back, and the historical connection between our extensive conflagrations, and the appearance of our epidemical yellow fever will soon be evident. Our most destructive seasons have been those of large fires. It is unnecessary to go further back than the year 1838, which if it did not originate, certainly aggravated the fever of that and the subsequent year. With the occurrence of large fires, exposing a fresh surface to high solar heat, with excavations for drains or any other purpose, with the diffusion of the refuse of kitchens and yards to form streets and lots, we may safely connect seasons of unhealthiness, if the state of the atmosphere favours the production of malaria. The agency of heat and moisture is essential to the effect, but without the material supplied by filth, by animal and vegetable decomposition, no mischief will follow. So that we may open our drains, we may excavate acres of soil, and scatter their imbedded morbific matter on all sides, in an excessively dry or excessively moist season, or with a low summer state of the thermometer, without peril. But having done this with impunity for a number of years, under favourable atmospherical conditions, we are too ready to lose sight of the great lesson which the reverse teaches."

These extracts are sufficient. They show that Charleston *ab urbe conditâ*, had local causes, which, added to the great solar heat, humidity and high dew-point, were adequate to produce malignant fevers; and are accordingly found, in her early history, that such fevers did prevail. We find, too, that the patriarchs of the profession, and others, from the days of Dr. Chalmers, about the middle of the last century, to the present, a period of one hundred years, have often warned the city government and citizens of the dangers arising from the narrow lanes and alleys, the confined houses and lots, crowding, bad ventilation, defective drainage, filth, and every variety of city mis-

management; and generally, the authorities and inhabitants have disregarded these warnings. In 1849 and 1852, yellow fever years, the measures of the city government in relation to sanitary police were so repugnant to the dictates of ordinary prudence and common sense, that they can scarcely be accounted for.

We will survey the city at the present time, commencing with East Bay Street, on the east side of the town, bordering on Cooper River. This and Elliot Street were paved in 1819, being the first streets ever paved in Charleston, before which time it was no uncommon thing to see drays *stalled* or *set* in the middle of the street in a wet season. These streets were sloughs in those days.

Let one pass from the Battery up East Bay Street, and he first comes to Stoll's Alley and Longitude Lane, both running to Church Street, and both filthy, but paved; then to Tradd Street, the foot of which is always crowded and dirty; then to Elliot Street, adjoining which is Bedous Alley, both of which would be an acquisition to the New York "five-points;" and then we come to Broad, the Wall Street of Charleston. Passing north, we come to Unity and Lodge Alleys, both narrow, crowded, and dirty; nor must Queen Street be omitted, which is crowded and always dirty; and then to Amen, Linguard, and Market Streets, all of which are always dirty. Market Street is invariably filthy, winter and summer. The "putrid cabbages and vegetables" mentioned by Dr. Shecut are always in perfection; and it is surprising that malignant fever is not present annually. The street is invariably filthy, from beginning to end, from the wharf to Archdale Street, in all seasons, emitting unpleasant odours even in mid-winter, and in summer the effluvia are intolerable. In October, 1852, when the yellow fever was severe in Charleston, I met a gentleman who inquired if I was not afraid of taking the fever by coming to town? My reply was: "I do not see why I should take it, for I have been among yellow fever during the whole morning in my own hospital, and I cannot see what harm there is in walking your streets for an hour or two in mid-day; but if you would pay me fifty thousand dollars I would not sleep in Market Street to-night." Crossing Market Street, we come to Pinckney and Guiguard Streets, with their close houses and filthy yards, two of the dirty streets of Charleston, in the vicinity of Mott's Lane, and the famed Union Wharves and "Hard Alley;" we then pass Hazell and Wentworth Streets, then Bennett's Mills, and the marshes, abounding in rotting wood; and then passing Society Street, at the foot of which there is a marsh lot into which a sewer disgorges its contents, among rotting wood and filth of every kind, we at last arrive at Lawrence Street. From Lawrence we pass to Calhoun, formerly Boundary Street, which divided the city proper from the Neck. Between Lawrence and Calhoun Streets is a square, bounded as follows: North by Calhoun Street, east by Cooper River, south by Lawrence Street, and west by East Bay Street; and containing Washington, Vernon, Inspection, Marsh, Concord, and Wharf Streets, all in the most filthy condition, with numberless filthy alleys and courts; and east of all these are wharves, among marshes, rotten wood, and filth of all sorts. The east end of Calhoun Street, toward Cooper River, is in the most filthy condition. East Bay Street ends at Calhoun Street, and crossing the latter we pass through Washington Street to a creek and marshes; then through parts of Chapel and Judith Streets to America Street, and thence to the city lines. The whole of the Neck, every street, in fact, bounded by the city lines on the north, Cooper

River on the east, Calhoun Street on the south, and Meeting Street on the west, is unpaved, undrained, dirty, and in very many parts, filthy.

The greatest part of the city proper, between Meeting Street and Cooper River, from South Bay to Calhoun Street, is dirty, often filthy, and generally insalubrious, and simply naming them will be sufficient; as Church, State, Anson, Elliot, Queen, East Bay, Market, Pinckney, Linguard, Guiguard, and Philadelphia Streets, the last known as "Cow Alley," a little nasty lane, filled with dirty tenements and dirty tenants, fit only to engender malignant fever; and the same remark will apply to Bedous Lane, Mott's Lane, Union Wharves, and "Hard Alley," which leads from East Bay Street down to one of the aforesaid wharves. These streets and alleys were all considered unhealthy in 1849 and 1852, and in all epidemic seasons they are decidedly so. This is the district, "from Church Street to the bay, with the intersecting alleys," of which Dr. Shecut says, that in "nine seasons out of ten, the prevailing fever originated within the limits of that section of the city." On the east side of East Bay Street are the wharves and docks, with mud, the contents of sewers, animal and vegetable substances in a state of putrefaction, and all kinds of rubbish and filth.

We have now gone west as far as Meeting Street, which is, all things considered, the best street in Charleston; and we will now proceed up the fashionable promenade, King Street, commencing at South Bay.

At the lower part of King Street, from South Bay to Lamboll Street and Smith Lane, there are dirty lots, some of them vacant, which are in bad condition in every point of view; then we come to Lamboll Street, and Smith Lane, both dirty, the last being unpaved, undrained, narrow, crowded, and filthy; and hard by this lane, on the east side of King Street, are two narrow alleys each leading into a little court, or *cul-de-sac*, and each crowded and in the highest degree filthy; and then we come to Weims Court and Princess Alley. All of these courts, lanes and alleys, are close, badly ventilated, crowded with foreigners and negroes, and filthy in the highest degree. None of them are paved, or drained, and the houses are in a state of decay. Crossing Tradd Street, we come to Broad Street, next above which is Queen Street. Queen Street, in its whole length and breadth, from Cooper River to Ashley, is very dirty, badly and only partially paved, badly drained, filled with worthless, decaying wooden buildings, filthy courts, and in epidemic years is considered very unhealthy. Next, north of Queen, is Clifford's Alley. Words will not fairly describe this nursery of pestilence.

Proceedings of Council, February 14, 1854:—

"The Aldermen of Ward No. 4, to whom the petition of certain owners and residents of property in the vicinity of Clifford's Alley has been submitted, praying that said alley be either paved or macadamized, in order to abate the nuisance now existing, and to remedy its almost impassable condition, beg leave to state that, even in the event of their reporting favourable to the above petition, it would by no means remedy the evil complained of. The offensive condition of this alley has, for a series of years, been a continual source of complaint, and the attention of the city authorities has been frequently called to the infectious, filthy state of many of the lots on the north side, located in the very midst of the city, immediately contiguous to one of the popular business streets, of valuable stores on each side to a considerable extent, &c."

Then come Horlbeck Alley and Clifford Street, both very narrow and very dirty; then Beresford and Princess Streets, both very dirty; and then the notorious street before mentioned, Market Street. From Lamboll to Market Street, inclusive, not a decent street, lane, or alley leads from King Street, on either hand, except Broad and Tradd Streets.

We will now proceed from Market, up King to Calhoun Street. First comes Beaufain, which is unpaved, badly drained, and dirty; and the lower portion, near Ashley River, is still worse, being made up of city offal, rice-chaff, and sawdust. Then come Hazell, Wentworth, Society, Liberty, and George Streets, most of which are in a pretty good state, but might be made better, some of them being badly paved and drained. And then we come to two notorious little openings, Burns Lane, and a little higher up, McBride Lane, both of which vie with Clifford Alley and Mott's Lane in everything disgusting. It is impossible to describe the bad ventilation, the want of drainage, and the intolerable filth of the lots, courts, tenements, inhabitants, and everything about them. Finally, we come to Calhoun Street.

This street, the old boundary street, is wide, and the centre of it in the vicinity of King and Meeting Streets, is pleasant, well-ventilated, and salubrious, though not yet paved, or well-drained. But other portions of the street bear a different character, the Cooper River end having been already described as intolerably filthy. The western part of the street, as we descend towards the low grounds on Ashley River, is undrained, and after heavy rains the water stands in the gutters until it gradually sinks into the earth, or is evaporated. This end of the street is very filthy. It is made land—made, as usual, on Ashley River, of city filth, rice-chaff, and sawdust.

Let us continue up King Street. Above Calhoun, on the Neck, are Vanderhorst, Hudson, Warren, John, Radcliff, Ann, Mary, Morris, Reid, Woolfe, Cannon, Spring, Columbus, Line, and Shepard Streets. None of these streets are paved, King Street having a pavement for only a short distance above Calhoun Street. They are wholly undrained, and after heavy rains in summer the water stands in them until it becomes stagnant, green, putrid, and emits unpleasant effluvia. These streets have not a good police, but some are in a much better state than others; and several are really filthy, as John, Ann, Mary, Morris, Reid, Wolf, Columbus, Line, and Shepard Streets. The last named compares very well with Clifford Alley, but being almost out of town, the filthy nuisance is not so injurious, and can be more easily abated. Cannon and Spring Streets may be considered in a terrible state of sanitary police, and insalubrious; though wide, they are low and flat, and wholly unpaved and undrained, the consequence being, that the standing water soon becomes stagnant and putrid in the hot season. In truth, the same remark applies to the whole of them, and to almost every street on Charleston Neck. Another nuisance must not be overlooked. The railroad occupies the position of a street between King and Meeting Streets, from the depot on Hudson Street to the city lines; and I am sure every one will agree, that the whole line of the road within the city is in the highest degree filthy. Standing water, green and putrid, exhales an offensive odour; and the vicinity of the whole track seems to have been made a receptacle for rubbish, decaying wood, and filth of every description. This road-track has always seemed to be capable of evolving more miasma than half a dozen of the offensive streets in the vicinity, bad as many of these are; and it would seem that the only remedy is to rip up the rails, and fill the whole track, not with city filth, sawdust, or rice-chaff; or drain it thoroughly, in connection with the adjacent streets and filthy lots; or finally, to entirely break up the road, transferring the depot beyond the northern boundary of the city. The railroad street is a nuisance which ought to be abated.

One thing more in relation to King Street. Besides not being in the best police generally, there is another evil which is liable to pass unnoticed. There are numerous private narrow alleys on each side of the street, from South

Bay to Calhoun Street, which are often closed by a nicely painted door, where everything appears to be in good order; but, on entering, you find yourself in a *cul-de-sac*, surrounded by little tenements or kitchens, with kitchen slops and filth of various kinds in abundance. These narrow passages are much more numerous than would be expected, and they are liable to be overlooked. Such is the condition of King Street and its purlieus in relation to sanitary police, and it is surprising that malignant disease does not occur in all summers. If such a state of things is found in the most fashionable and popular street of Charleston, what can be expected of the by-streets, lanes, and alleys?

We will now make a brief survey of the west part of the Neck, from King Street to Ashley River. This division of the city is low, flat, wholly unpaved, undrained; the streets are uncared for, the lots are filthy, many of them being occupied for cow pastures, piggeries, &c.; the houses in most of the streets are small wooden buildings, new and old, and altogether, this part of the city is in bad condition. Many of the low lots and streets near Ashley River have been filled with city offal, rice-chaff, and sawdust.

We will now look at the portion of the city bounded on the north by Calhoun Street, on the east by King Street, on the south by South Bay, and on the west by Ashley River. This part of Charleston is generally made up of narrow streets, which are badly ventilated, sometimes unpaved, badly drained, and often (generally) in the most filthy state; the tenements are small and crowded; the lots are small, and in the worst sort of police; and everything seems to be neglected. The whole west side of the city appears to be made land. Calhoun Street, below King, is wet, unpaved, undrained, having old wood buildings, filthy yards, and is filled up in the usual way. From Calhoun Street, and above to Broad, the streets and lots bordering on Ashley River have been filled up—literally made—with city offal, sawdust, and rice-chaff. In passing from Calhoun to Broad Street, near Ashley River, many of the streets tremble under the foot as if one were treading on a bog; and there is a bog, formed of city filth, rice-chaff, and sawdust. From Beaufain to Broad Street, inclusive, the streets and lots are filled, or are filling, with wood, rice-chaff, and city offal; and along this side of the town there are numerous lots with high board fences. Dr. Ramsay noticed the high fences in his day, and says that "the evils of a crowded population are increased by high and close fences, which are daily increasing;" which is doubtless true, for they not only obstruct the free circulation of air, but conceal unutterable filth; and the higher, the more closely put together, and the more neatly whitewashed the fence, the more reason is there for suspicion, and the more surely shall we find a mass of concealed filth within the inclosure. The western part of the town, from above Calhoun Street to below Broad, including (in addition to those before named) Savage, New, Mazyck, Logan, Friend, the west part of Tradd, Legard, Lamboll, and the foot of King, are made streets; and in the southern part of the city, in the vicinity of South Battery, the made land includes, besides Lamboll and King, Smith's Lane, Winn's Court, Price's Alley, many dirty little courts, alleys, and lots, and the south ends of Meeting and Church Streets. Most of these streets, alleys, and lanes, and very many of the lots are filled with refuse wood, rice-chaff, and from the scavengers' carts; and they are all wet, dirty, and in a bad state every way. Atlantic Street and Lightwood Alley, the precise site of the old Lynch Lane, which was the seat of yellow fever in 1817, are made streets—and so is Water Street. Indeed, whoever consults Dr. Ramsay, will find that much of the eastern, or Cooper River side of the town, has been filled up—as the marshes along East Bay Street, the eastern part of Queen Street,

and Market Street; and rice was planted in State Street about the middle of the last century.

What effect has this filling up of streets and lots? If filled with the right kind of material, as sand and gravel, the salubrity of the city would doubtless be immediately increased; but if every kind of perishable material, animal and vegetable, is to be used, the good effects must be remote and prospective, and the immediate effect is pernicious. Even after a long course of years, when excavations are made in the streets, lots, and along the wharves, where such unwholesome materials have been used for filling, especially when these excavations are made during the hot season, the consequences may be serious, from the abundance of pestilential effluvia given out while in this way exposed to the sun; and in this way were the extensive excavations of 1849 and 1852 so injurious. No prudent city government would attempt such things in midsummer; and the results were, as might have been expected, deplorable.

The filling of lots with refuse animal and vegetable material continues to be permitted by the municipal government.

"After a careful examination, the Board report that all of said lots contain stagnant water; and regarding the same as prejudicial to health, recommend to the City Council to cause said lots to be filled with earth, and drained according to law, within three weeks from notice, under the direction of the City Register."

"N. B. The lot of Mr. Green being very large, the Board would recommend that he be allowed three months to fill, and that he be allowed to use chaff or sawdust, provided the same be covered with three inches of earth." Concurred in.[1]

Three inches of earth! As well might they scatter the contents of a pepper-box.

"We would respectfully beg leave to call your attention to the necessity of extending the drain at the foot of Market Street beyond the proposed location of the new fish market."

"We shall soon be ready to remove the earth from the interior of the basin, and it is proposed to use the same in filling in of the open space between the basin and street; to effect which it will be necessary to construct the extension of the drain."[2]

And the "filling in" with dock-mud is proceeding!

The butcher-pens in the upper part of the city are nuisances, and petitions have been presented against them to the city council.

We have occupied much time in the consideration of a long list of evils, but in connection with bad ventilation and bad drainage, both strictly coming under the head of sanitary police, it is scarcely possible to say too much. The importance of the subject warrants, if anything can warrant prolixity. After having given the opinions of Drs. Chalmers, Ramsay, Shecut, Simons,

[1] Proceedings of Council, April 26, 1853—Report of a Board of Inspection on Lots in Calhoun and Nassau Streets. * * *

[2] Proceedings of Council, February 14, 1854—Report of Committee of Market Board.

and Hume, and also the opinions of the writers in the *Charleston Evening News*, all in their own language, I trust I shall be excused for saying, what every one of these writers has in fact said, that the city is in a bad state of sanitary police, and that a change of system is imperatively demanded.

On Sullivan's Island there is really no sanitary police. From the 1st of May to the 1st of November, a small allowance is made by the village authorities for the purpose of removing the putrefying carcasses of animals, which float down the river past Charleston, and are carried by the current and tide on to the south side of the island; and these are usually got off in a putrid state. But, from the 1st of November to the 1st of May, people are supposed to have no nostrils, lungs, or stomachs, and the bodies of dead animals are not removed. There are numerous small and crowded houses, in addition to the negro kitchens, which are occupied by Irish and other families, who are more filthy in their habits than the negroes; and for years, the filth of all sorts, kitchen slops, and everything which can be mentioned, has been cast abroad, or allowed to accumulate. The privies are another nuisance. They are literally above ground, and in certain localities, in the heat of summer, the effluvia are very offensive. During the heavy rains in 1852, the water in the vaults, where there were vaults, was on a level with the surface of the earth; and north of Middle Street, towards the back beach, the high tides floated out the contents of several, spreading them over the adjacent soil. The contents of the privies, with other filth, in such a porous soil as this, must undoubtedly affect the water to some extent, and in every respect contribute to insalubrity. Lime, chloride of lime, and other disinfecting agents, should be freely used, which seems not to have been the case. No privy ought to be constructed in such a porous soil as this without a vault of brick or stone work, rendered water-tight by cement.

8. *Bad Water.*—This is a serious matter to the inhabitants of Charleston. For many years there have been great complaints for want of this necessary article of existence. Dr. Ramsay mentions the want of good water at length in his *History of South Carolina*, and proposes two remedies for diminishing the heat of the city: shading the streets by trees, and the "plentiful introduction of water, so as to give a facility for washing and cooling the streets with an artificial shower whenever wanted." Again:—

"The daily removal of putrescible substances lessens the evils of an impure air, but is inadequate to the purpose intended. The only effectual remedy is fresh running water. This unites cleanliness with coolness."

Dr. Shecut says:—

"From the situation of Charleston, there were but few springs affording wholesome drinking water; hence, for many years, the citizens were obliged to drink that which was collected in wells sunk eight or ten feet below the surface of the earth, which is a little above the level of the surface of the low tide, and the quantity of water contained in them is usually from four to five feet, which is also nearly on a level with the high tide. These, together with the nature of the soil, occasions the water to be generally brackish; to strangers, extremely

nauseous, inducing nauseous gripings, and even troublesome diarrhœa."—P. 78.

It may be contended that Charleston is in as good sanitary condition as the city of New York, or even much better; but that is not sufficient; for it is to be recollected that a better system of sanitary police must obtain for every month of the year than in any northern city; and to assert that the police of Charleston is as good as that of any town at the North, is saying very little indeed. Our cities are not by any means examples of good sanitary police; on the contrary, scarcely one of them of any size, except Boston and Philadelphia perhaps, have even a decent system of police; but they rarely engender yellow fever, while Charleston, with only the same local causes, may ordinarily engender the disease, and periodically does engender it.

The well-water of Sullivan's Island is brackish and unwholesome, and cisterns to the dwellings are as scarce and as valuable as diamonds from Golconda. Few of the houses have them, insomuch that a cistern is an exception to the general rule. The wells, water, &c., have been sufficiently spoken of under the head of drainage. The wealthy families, who reside on the island only in the summer season, are able to procure pure water in the neighbourhood; but those who need it most, the poor, the miserable, and the degraded, who remain during the whole year, cannot get it. The water at Fort Moultrie, for drinking, culinary purposes, and washing, is cistern water, of which there is abundance during the rainy season; but in a time of drought, the garrison has to be put on short allowance, the brackish water of the wells having to be resorted to for general purposes.

9. *Heavy Rains.*—These were a cause, predisposing and exciting, of the malignant fever of 1852, both in Charleston and Sullivan's Island, producing, in combination with other causes, yellow and severe bilious fevers. The summer of this year was very wet, the quantity of rain in each month being as follows:—

	Charleston.	Sullivan's Island.
May, . . .	4.22	4.17
June, . . .	5.18	8.86
July, . . .	6.93	5.43
August, . . .	4.21	4.15
September, . .	12.27	11.70
Total quantity,	32.81	34.31

Whole quantity for the year: Charleston, 49.72 inches; Sullivan's Island, 51.26 inches. The quantity of rain which fell in Charleston is taken from the tables of Dr. Dawson, city register. From the records, we see that, both in the city and on the island, about two-thirds of the whole quantity of rain for the year fell in the five hot months, every one of which had more than an average quantity; and that by the first of September the earth was fully saturated, when 11 or 12 inches more were added, most of it in the first twelve

days of the month. The register of Fort Moultrie shows that, during the first twelve days of September, 9.40 inches fell, and from the 7th to the 12th inclusive, six days, there was no less than 7.80 inches, 1.30 inches per day, the earth being previously saturated; and it must be recollected that the epidemic broke out in a decided form on the 7th and 8th of the month, and that the first case of yellow fever occurred on the 11th, showing evidently that these heavy rains were the exciting cause, acting on systems predisposed to disease by heat, great humidity, and a high dew-point, by which malaria is rapidly evolved, and all acting in conjunction with other evident causes of disease. Perhaps the matter was rendered still worse by the dry weather and rapid evaporation which followed, for there was not a drop of rain from the 25th of September to the 9th of October, nor from the 14th of October to the end of the month, the whole of October being very fair, with only 1.20 inches of rain. From the 15th to the 25th of October, inclusive, at every meteorological observation, four times a day, the clearness of the sky was 10, which denotes clear sky, without a cloud; prevailing wind, northerly. During the pouring rains of the first part of September, the whole country was nearly submerged, in consequence of which, various types of fever prevailed; and on Sullivan's Island, bilious and yellow fever were universal, the former being very common indeed, so much so as to merit the term universal, the only healthy spot in the vicinity of Charleston being the barren and circumscribed post of Fort Tumter. Cases of "country fever" occurred at the village of Mount Pleasant. Under the head of drainage, the condition of Charleston and Sullivan's Island, in this year, was sufficiently shown, and more is not required.

We have considered the principal apparent causes of yellow fever in 1852, which appear to have been the same as those of previous epidemics; and they are, 1: intemperance; 2, fatigue and exposure; 3, imperfect ventilation; 4, high solar heat; 5, humidity, and a high dew-point; 6, defective drainage; 7, bad sanitary police, which includes animal and vegetable decomposition; 8, bad water; 9, heavy rains.

These different causes are of more or less importance in the production of disease; for yellow fever may occur (as in 1852) in persons who are not intemperate, and have suffered no exposure or fatigue; and, therefore, these cannot be considered essential causes; but there are others which are essential, without which the disease cannot exist, which are unavoidable, and always present; as high solar heat, humidity, and a high dew-point; and there are important causes which are not unavoidable, and are removable, but, acting with the unavoidable causes, generate malaria or the *materies morbi* with wonderful rapidity; as bad ventilation, defective drainage, and a bad sanitary police generally, and which should be obviated without delay, in order to neutralize the influence of the unavoidable causes. Good drainage will remove another cause of disease, heavy rains, by running the water rapidly from the surface to the rivers and harbour, and thus it will be prevented from becoming stag-

nant and putrid. Yellow fever seasons are not necessarily wet, though a majority of them have been so; Dr. Shecut says, in the proportion of four to three. It is not always easy, however, to determine what quantity of rain constitutes a dry, and what a wet year; for a wet summer and a wet year are very different, in regard to salubrity, as is well explained by Dr. Shecut.

"A fall of 20 to 25 inches in the year, if it happens within the four months which mark the sickly season, will, as it regards the health of the city, constitute a *wet summer;* while, on the other hand, if there be a fall of 30 to 45 inches in the year, provided there does not fall more than 2½ to 3½ inches in each month of the year, the distribution being thus equalized throughout the twelve months of the year, the season is properly a *dry one.*

"Although less rain fell in the year 1802, than in any of the seven preceding years, and though there were only sixty-four days in which an actual fall of rain took place, thirty-six of these days were in the three sickly months of June, July, and August, in which there fell at an average of 7 inches in each month, or 21 inches in thirty-six days of the three months, and which, happening at that season of the year, commonly known as the sickly season, must, as it regards the medical history of the city, be called a *wet year.*—Pp. 63, 64.[1]

There is a certain something behind all these evident causes of disease—no one knows what it is, and it may never be known—which is essential to the production of yellow fever; it may be the state of electricity, or it may be a deficiency of ozone in the atmosphere;[2] but it is certain that, in the city of Charleston, there has always been high summer heat, great humidity, and a high dew-point; and that, for several years, there has been bad ventilation, bad drainage, and a bad sanitary police, both in wet and dry summers, yet yellow fever has not prevailed in every season in which all these conditions were present. Dr. Hume was so well convinced that every cause of yellow fever was present in Charleston, in 1853, that in the first part of the year he announced the fact in the city newspapers, prophesied the occurrence of the disease, &c.; but, after all, no fever came, the year proving as healthy as almost any one of which we have records.

Whatever may be the ultimate cause of yellow fever, we will give the following extract from Dr. La Roche, on malaria:—

"The digging of canals, the opening of ditches, the cutting down of bluffs, the levelling of lots, the filling up or digging down of streets, the opening of roads, the establishment of brick-yards, &c. have proved highly injurious, not only in this country, but in parallel or hotter latitudes, as also during the hot seasons of cold regions. The results of operations of the kind in the East and West Indies are on record, and, like those in Algeria, an account of which has recently appeared, are of a nature not to be easily forgotten. Near Tlemcen, in Algeria, nearly all the soldiers employed in digging a well were attacked with fever, while all the others in the vicinity escaped. The opening of roads,

[1] Dr. Ramsay gives 39.10 inches as the whole quantity of rain for the year 1802, but during the first three months of that year there were only 2 inches; 2 more in April; May, 3; June, 3.40; July, 12.10; August, 4.95; September, 5.85; 29.30 inches from May to September, inclusive—an average of 5.86. Yellow fever raged in Charleston this year.—*History of South Carolina,* vol. ii. pp. 57, 85-86.

[2] Dr. Hume supposed that he had "found in the store of Mr. Gatchell, No. — Market Street, between Church and State Streets, and adjacent to Linguard Street, in a part of the city remarkable for the prevalence and fatality of the fever," the real miasm of yellow fever.—*Charleston Medical Journal* for January, 1850, pp. 24, 25, and 26.

and the upturning of the earth for various purposes, are recognized, by the entire body of physicians attached to the French army in that country, to have given rise to the extensive development of fever. The writings of Dr. Drake, so often cited, those of Evans, Blane, Cassan, Macculloch, Usher Parsons, Monfalcon, Caldwell, Baley, Thomas, Anderbach, and many others, abound in similar cases. Dr. Merrill, in an essay on the yellow fever of Natchez, has taken great pains to show, and with every appearance of success, that the epidemic of 1823 arose from the levelling of the streets, and the consequent exposure of the fresh soil to the action of the hot sun."

Almost every line of the above-cited passage is applicable to Charleston. The digging of canals, the opening of ditches, the levelling of lots, the filling up or digging down of streets, the opening of roads, and the upturning of the earth for various purposes, as noted by Dr. La Roche, remind one strongly of the filling up of lots with city offal, the opening of drains, the laying of gas-pipes, the filling in with dock-mud, the excavations for the new custom-house and in other places, and the upturning of the earth in numerous localities, in Charleston, during midsummer in 1849 and 1852; and the "levelling of the streets, and the consequent exposure of the fresh soil to the action of the hot sun," at Natches, in 1823, as shown by Dr. Merrill, bring vividly to recollection the extensive excavations in the summer of those same years of 1849 and 1852, when the filthy and offensive earth was thrown up to the action of the *hot sun*, the abominable material having been actually transferred from one street to fill up another.

A consideration of the causes of yellow fever brings us directly to the subject of contagion. (*To be continued.*)

ART. VI.—*Extracts from the Records of the Boston Society for Medical Improvement.* By WM. W. MORLAND, M. D., Secretary.

May 8. *Dropping of Fluids from Vials; a good Method.*—Dr. COALE mentioned that he had found a very simple and ready means of remedying the frequent difficulty of dropping fluids from a vial. Unless the lip projects very much, the fluid is apt to run back on the neck, and remain attached until several drops have accumulated. Greasing the neck of the vial (not the lip) will prevent this, and give a clean, uniform drop.

Encysted Tumour on the Forehead: Disappearance, after puncture of the cyst and irritation of its internal surface.—Dr. COALE had a case of common encysted melicerous tumour, on the forehead of a young lady. Unwilling to make a scar in so conspicuous a place, he passed a needle under the skin, at some little distance from the tumour, and then into the tumour itself, moving it about there and scratching its internal surface. This operation having lessened the tumour very much in size, he repeated it three weeks afterwards, and was gratified to find it in a short time entirely absorbed.

[1] Charleston Med. Journ. for May, 1853, p. 304.

Ophthalmoscope.—Dr. BETHUNE showed an ophthalmoscope, made under his direction by a son of Mr. ALVAN CLARK, of Cambridge, from a description in a recent letter from Paris, published in the *Boston Medical and Surgical Journal.*

It consists of a concave disk of glass or speculum metal, with a focus of eight inches, of about the size of a dollar, perforated by a funnel-shaped hole in the centre, as large as a goose-quill. The brass case serves for a handle. The observer, by throwing the rays of a candle into the pupil of the eye, after dilatation, gets a very good view of its internal appearance through the orifice, from behind.

Caries of Elbow-Joint.—The specimen was shown by Dr. J. MASON WARREN. The patient was a female, 19 years old, of a scrofulous habit. Two years ago, she was under medical treatment for some difficulty in the lungs. Eighteen months since, a swelling appeared near the elbow-joint, which gradually increased to a large size, when it was opened, discharging a great quantity of pus. No dead bone could be detected with the probe. The forearm remained for a time fixed in a bent position on the arm, but suddenly, four months ago, in a single night, became extended, and has hung in that position, entirely immovable and useless, since. Her general health had become much affected by this constant source of irritation and pain. At a consultation held on her case, it was at first decided not to amputate the limb, on account of the affection of the lungs, which was evidently tuberculous; but at a second examination of the case, two months subsequently, it was determined to remove the limb, *especially as the patient herself strongly urged it.* The arm above the swelling was quite small, yet a number of large vessels required ligatures, evidently going to supply the swelling below.

Since the operation, the patient has improved; and now, at the end of two weeks, is able to be up and go down stairs; her cough less, and all the functions are performed naturally.

An examination of the joint disclosed very extensive disease. The cartilages on the head of the bones were entirely destroyed. The fibro-cartilage, on which the radius revolves, had disappeared. The joint was filled with pus, and sinuses extended from it in two or three directions. All the muscles in the neighbourhood had undergone the fatty degeneration.

Blighted Fœtus and Placenta—Suspicion of Syphilitic Taint.—Dr. PUTNAM exhibited a blighted fœtus and placenta. The mother has two children. Since their birth has had three miscarriages; one at the third month, one at the seventh, the fœtus having the development belonging to that period; and this last at the eighth month, though the vitality of the fœtus ceased at the third.

The husband stated that he contracted syphilis subsequently to the birth of the children; that he was immediately put under treatment, and, before any further conjugal intercourse, was considered free from disease. The mother had inflammation of the vagina, with muco-purulent discharge, which readily yielded to treatment. Neither has had any local or constitutional sign of disease whilst these imperfect gestations have been recurring.

Dr. Putnam supposed that, in this and similar cases, abortion might be attributed to the ordinary sympathy of the uterus with any grave disorder; but he also thought it not improbable that the specific poison, once having made its impression on the maternal system, might be operative, after all its appreciable signs had disappeared, and might affect successive products of conception.

Dr. COALE said that, from the experience of others, and from his own, he was always induced to suspect syphilitic taint in morbid alteration of the placenta, like the one just exhibited, where much, if not almost the entire bulk of the organ becomes solidified. Besides other cases, in which cause and effect were too obvious and evident to admit him to doubt for a moment the course to pursue, Dr. C. had one case which he thought an interesting one, on several points. He was called to attend a fine, healthy-looking English lady, who had already a hearty child, some three or four years old. She was anticipating, in full confidence, great pleasure from the prospective increase of her family, and when a stillborn, half-putrid child came into the world, her disappointment was proportionate. Within twelve months, the same promise was held out, to be disappointed in the same way, and this time with great effect upon the spirits and health of the mother. Dr. C.'s suspicions were now aroused as to the probable cause of the state of the placenta and death of the fœtus, and he had the trying and unpleasant duty of propounding it to the father. He confessed that, some three years before Dr. Coale's acquaintance with him, he had for a short time been irregular in his habits, had contracted disease, but was not aware of giving it to his wife. Investigation into the case of the wife gave no evidence of chancre, but decided evidence of secondary eruption, which had continued even to the time of examination. Dr. Coale put her at once on the deutiodide of mercury, affected the gums, very slightly, twice, and within another twelve months had the pleasure of delivering her of a fine hearty child.

In another case, however, Dr. Coale had witnessed the same change in the condition of the placenta, and the consequent death of the child, of a healthy primipara, in which no evidence could be found to awaken even a suspicion of syphilitic taint. The mother, in this case, was delivered a year afterwards, without any medical interference, of a fine healthy child.

Dr. Coale noted, in these cases, the very rapid decomposition of the child in utero. In the first, motion was perceptible until within forty-eight hours of delivery, yet the fœtus was a putrid mass. In the second and third cases, the child was evidently alive within twenty-four hours, yet the whole body was flaccid, the skull soft and shapeless, and desquamation had proceeded.

Abortion; Diseased Placenta, &c.—Dr. BLAKE remarked that he attended a woman in confinement, April 17; the child came away dead; the whole mass, placenta and unruptured membranes, passed at once. Six weeks previously, having taken exercise to an unusual degree, the patient was attacked by pain and had some flowing, which latter, however, ceased in two or three days, without any resort to remedial measures The afterbirth was found diseased throughout one-half, or more, of its extent, being converted into a semi-cartilaginous substance. There was no reason to suspect syphilitic taint.

[In cases in which syphilis has had an influence in causing abortion, &c.—as in those related by Drs. Putnam and Coale—Dr. W. TYLER SMITH, of London, has some interesting and important remarks in the *London Lancet* for May, 1854. He concludes that "where the placenta and membranes become so diseased as to cause abortion, the child remaining free from disease, the mother is pretty sure to be affected with the disease; but when the child is born living, and is apparently healthy at the time of birth, the mother may in some cases escape contagion." "When the ovum is affected by secondary syphilitic disease, we can easily understand that the blood of the fœtus should infect the mother through the placenta." "There can be no doubt, I think, that, in practice, in all cases of repeated abortion, and eruptions in the early

months of infancy, the health of the male parent, before marriage, should be strictly inquired into."—*Op. cit.* p. 425.

At a meeting of the *Harveian Society*, Mr. BORHAM referred to Dr. Smith's paper, proving the "transmutability of secondary syphilis from the father to the child," &c. Mr. B. also exhibited two fœtuses, lost at about three weeks from conception, and from the history of which, "as well as from two or three others unrecorded by him, he was induced to believe that many cases of abortion occur, and may be traced to the fœtus being vitiated, from having been begot at a time when the father is suffering from that blood disease, secondary syphilis. The mother's healthy uterus would expel it, as it would any other foreign body, from its being stimulated by an unhealthy, vitiated, and unnatural conception."—*Lancet, ut supra cit.* p. 469.—SECRETARY.]

May 23. *Tumour from beneath the Skin covering the Glutei Muscles.*—Dr. DURKEE exhibited a tumour sent to him by Dr. HENRY C. PERKINS, of Newburyport. It was removed from a clergyman about forty years of age; began to show itself twenty years ago, just below the skin; was always hard to the touch; never painful; produced no inconvenience except as being in the way while the patient was in a sitting posture. Tumour weighed four ounces and one drachm; consisted of epithelial scales, concrete sebaceous matter, carbonate of lime, and phosphate of lime;[1] no cholesterin. Its structure was laminated, and its shape nearly round. It was so hard and solid that it required to be sawed open for examination. The inner surface of the sac containing the tumour was lined with soft epithelium.

Professor Ackley's Case of Hermaphroditism.—Dr. J. B. S. JACKSON made the following remarks: An account of this very remarkable case has already been published in the *Am. Journ. of Med. Sci.* for July, 1853, by Dr. BLACKMAN, of New York; and in the following number of the same Journal is a report to this Society by the late Dr. BURNETT, of this city. Dr. Blackman's paper is accompanied with a wood-cut; casts of the specimen have also been made, coloured, and mounted for distribution; and a copy is in the Society's cabinet, sent by Prof. Ackley. If there is any question, then, as to the facts, anatomically, it is desirable that it should be made known. Dr. Jackson said that he had had an opportunity to examine the specimen recently, through the kindness of Prof. A.; and he thought that the accounts above referred to were in some important points questionable or incorrect.

The os tincæ is represented in the figure and in the cast as a prominent object. Dr. J. found no trace of it; but the uterus passed insensibly into the vagina. This last was nowhere of the full size, and towards the opening into the bladder, measured, at the smallest part, between four and five lines in circumference upon the inner surface; in the Society's specimen of the cast, something like transverse rugæ are represented, but nothing of the kind was seen. The fimbriated extremities of the Fallopian tubes were distinct enough, though very slightly developed.

The very important fact of the existence of ovaries when the testicles were so well developed, should be well attested. Dr. J. found some thickening of the tissues about where the ovaries should be, but it was ill defined and slight; and it would not have been thought of, except in connection with the present question. Upon one side, an incision was made into this questionable part; but nothing like a Graafian vesicle was seen, nothing but a loose cellular or fibro-

[1] The phosphate of lime was detected by Dr. Bacon.

cellular tissue; a yellow body was exposed, which was thought at first to be a corpus luteum, but it proved to be a lobule of fat. Upon the other side, no incision seems to have been made.

One of the testicles had been cut open, and its structure was quite normal; the other had scarcely been cut into. In regard to size, Dr. J. thinks that they were larger than they have been stated to be by Dr. Burnett; his attention, however, was not particularly directed to this point, as he had forgotten Dr. B.'s statement, and did not have an opportunity to refer to it until after his return. Some allowance may, perhaps, be made for the circumstance of the specimen having been in spirit for a number of years. Neither of the organs was dissected so as to show the epidydimis, though there could be no doubt of its existence. An incision was made upon a cord that had the feel of a vas deferens, and it was very nearly exposed, midway, to the extent of about two inches; towards the testicle, it appeared to diverge from that organ, so far as could be seen by holding the parts up to the light; but, as no dissection was made, Dr. J. would not say that it was not the vas deferens, and that it did not arise from the epidydimis. Towards the neck of the bladder, this cord could not be traced. Upon the other side, something was felt indistinctly that might have been a vas deferens.

The vesiculæ seminales were not found.

In the situation of the prostate, there was a feeling of resistance, as if the gland might exist; but it had not been demonstrated.

The penis, scrotum, and other parts, appeared to Dr. J. as they have been already described.

Professor Ackley made a longitudinal incision through the uterus, and very kindly gave permission to Dr. J. to examine, by dissection, several points about which there seems to be a question. Dr. J., however, declined the offer, in the hopes that so interesting a specimen would be examined thoroughly by some professed anatomist.

Sudden and complete Cessation of Premature Labour-Pains.—Dr. COALE was called to a young girl in a state of extreme narcotism, as was afterwards ascertained, from an intentional overdose of paregoric. He discovered she was some five months gone in pregnancy. She was relieved, but Dr. C. was called again within forty-eight hours, and found her apparently in violent labour, the pains regular, and so severe as to cause violent straining, clutching at the bedpost, and bearing down; in short, all the symptoms of severe, active labour. He left word to be sent for again; but receiving no message before the next day, he again visited his patient, and found, to his astonishment that, as the night advanced, the pain left her without delivery taking place.

That partial and irregular contraction in the muscular fibre of the uterus near the full term might occur without expulsion of its contents, may not be very strange; but that such violent contraction should take place in a uterus containing a five months' fœtus, and not expel it, Dr. C. thinks worthy of note.

Passage of Air per Vaginam after Delivery, before the Removal of the Placenta.—Reported by Dr. E. W. BLAKE. May 13, Mrs. S. was delivered of a daughter, weighing nine pounds, after a rapid and easy labour. The funis having been tied and the child removed, whilst standing by the bedside, a few moments afterwards, awaiting the throwing off of the placenta, Dr. B.'s attention was attracted by certain peculiar sounds, emanating from the person of the

patient. His first thought was that the desired contraction of the uterus had forced after-birth, coagula, &c. from the vagina, and that their forcible expulsion through this passage had given rise to the *phenomena*. An examination, however, showed that the after-birth still remained, but revealed the real source of the *sounds*. As the hand reached the os externum, the noise was renewed, now affording sensible proof, by the breaking around the fingers of *bubbles of air* forced through the mucus, &c. of the vagina, that their origin was within this passage, and that the *explosions* which continued (audible and evidently annoying to the patient herself) during two or three minutes, followed the uterine contractions. The after-birth was now found to be detached, and as a finger drew down one edge, a sudden and final rush of pent-up (inodorous) air occurred. No other unusual symptoms accompanied this otherwise natural case.

A Precocious Infant.—Dr. BLAKE mentioned the following facts: May 11, attended Mrs. H. in her (fourth) confinement, after a tedious, though not severe labour; the child, a female, unusually vigorous, and weighing, at birth, eleven and a half pounds. On the third day, subsequent to visiting his patient, the nurse asked: "Doctor, do you see our baby laugh?" Incredulous, he crossed the room—but, to his surprise, the infant returned an answering smile to the caresses of the nurse. With a look of intelligence, it turned to the father who stood over it, as he patted its cheeks, and as quickly turned its eyes on Dr. B., when he did the same; and when he stepped aside, its gaze followed him (attracted, he supposes, by his *spectacles*) until, being out of view, it cried aloud; returning to its side, a smile of pleasure again greeted him, and the cry was once more repeated when he ceased to notice the little one.

Here was unmistakable evidence of pleasurable and sad emotions, and showing the distinct recognition of sensible objects, by an infant as yet scarce sixty hours old. That it was no accidental circumstance, the like manifestations, which were daily noticed afterwards, sufficiently prove.

Vagitus Uterinus: the so-termed Sound simulated, in a Case of Operation for Vesico-Vaginal Fistula.—Dr. PARKMAN said that, while performing the operation for the union of vesico-vaginal fistula, he had heard a sound which closely resembled that described as *vagitus uterinus* of the infant. In this case, the sound doubtless arose from motion communicated to the mucus and other secretions in the vagina.

Dr. BIGELOW, Sen., remarked that in foot and breech presentations he had frequently heard a child cry before the head was out of the vagina; the face presenting, so that the mouth was near the external air. He added that, in such presentations, after the body is delivered, if, on examination, the child be perceived to gasp, and, at the same time, kick convulsively, these are signs of danger to its life; if, at this moment, two fingers can be introduced, so as to draw down the jaw, and, by pressing back the perineum, admit air, the child will live; forcible attempts at delivery of the head will not then be necessary, and Dr. B. believes that the head will, in such cases, be naturally delivered.

June 12. Revaccination; Question as to its Necessity, &c. &c.—The question was raised by Dr. STORER, and a discussion ensued, in which several members took part; a condensed account of the opinions expressed is subjoined. Most of the gentlemen would revaccinate, *if requested* to do so;

several would certainly do the operation if persons were much exposed; certain members declared themselves *non-revaccinators.*

Dr. STORER observed that two cases had lately occurred in his practice, to which he would refer, with the hope of eliciting the opinions of the members of the Society respecting the subject of *revaccination;* that he might learn the views of the gentlemen present, regarding its propriety, expediency, necessity, and, if necessary, how often?

He felt that he already knew the views of many of his brethren; he wished to hear a free discussion from *all.* As so many cases of serious inconvenience, to use no stronger language, have occurred in this city during the few past years from revaccination, and as we are seldom called upon to revaccinate a patient unless a larger number of cases of smallpox are known to exist in the city than usual, he was in the habit of pursuing the following course. Whenever a person presented himself for revaccination, he inquired if he had recently been, or expected to be exposed to a case of smallpox? If his answer was in the affirmative, he vaccinated; if in the negative, he advised that the operation should not be performed.

Two or three months since, he was called upon to take charge of a case of smallpox. The wife of the patient had been vaccinated, years before, and her arm exhibited two well characterized vaccine cicatrices. She was disinclined to be revaccinated, and eventually suffered severely from varioloid.

Since this case occurred, a boy nine years of age, whom he had vaccinated four years previously, and upon whose arm were two perfect vaccine cicatrices, was attacked with smallpox, and died at the expiration of four days. This latter case naturally awakens the inquiry, how often should vaccination be resorted to? The common notion that it should be performed every seven years, would not answer. In this case the system had not been protected for four years. Ought we to consider this merely an exceptional case? Ought we, as a general rule, to advise revaccination? and if so, how often?

Dr. C. E. WARE reported that he had seen one case in a teacher, who had contracted the disease by visiting a house adjoining that in which a person was sick, and afterwards died, of smallpox. The disease was of so light a character that it did not confine him, and he, supposing that it was merely a cold, visited his pupils as usual. All his family, consisting of five members, all of whom had been vaccinated formerly, but not at that time, had varioloid. None of his pupils contracted the disease.

In four other families, Dr. Ware had recently had a single case of varioloid. Although some of the members of these families were revaccinated, most of them were not; and yet, so far as he had learned, although the period of incubation was long passed, there had been no second case of varioloid. In another family, of many members, although none were revaccinated, there were only two cases of varioloid.

Dr. INCHES desired, as the question of duty, by Dr. Storer, in his case of variolous disease had not been answered, that some advocate of revaccination would state the laws of the durability of protection by vaccination, and consequently the frequency of revaccination necessary to secure protection. This is important, first, because the whole population cannot be revaccinated at each invasion of smallpox, and secondly, because during epidemics of smallpox and erysipelas, severe general and local symptoms occasionally attend the operation of revaccination. Therefore, if revaccinations are to be repeated, it should be at periods insuring protection, and at moments when the above epidemics do not prevail. Dr. I. felt, however, that he did his duty when he tested each vaccination by an immediate revaccination.

In considering the question of the necessity of revaccination, Dr. COALE thought that there were several parts to the proposition, several previous questions to be answered. That the system is capable, after an interval, of a second, or even third impression by vaccine virus, has long since been shown by the result of revaccination in the Prussian army, and in Wurtemberg. The results are herewith given, and we have every reason, in the known high discipline of the Prussian army, to rely perfectly upon these statements. [Dr. C. here quoted the following paragraph from the *Medico-Chirurgical Review.*]

"*The Practice of Revaccination.*—Every medical man must be aware that the propriety or necessity of this practice has excited much attention of late, although its investigation has been unaccountably neglected by the profession in this country. To us, there seems no one valid objection to urge against it. It has been said, indeed, that such adoption would unsettle the public mind in its faith in vaccination. Nor need this be regretted. The most fatal condition of the public mind, and from which much evil has already sprung, is *apathy.* Let public attention be fairly aroused, the merits of vaccination will then undergo renewed discussion, and its more general adoption must be the result. It is especially to Prussia and Wurtemberg that we are indebted for the experiment of revaccination upon a large scale. In the former country, of 216,289 revaccinations during 1833–7, there were 84,516 successful; and of 44,000 in the latter country, 20,000 succeeded. Frequently, too, cases which failed on a first trial succeeded on a subsequent one. The precise proportion of successful cases has varied from 31 to 45 or 46 per cent.—the period between the ages of 10 and 30 being found that most certain of success. Of course, no one infers that the success of revaccination implies a liability to smallpox in an equal number of cases. The operation, in fact, in the hands of Heim, proved successful also in 32 per cent. of persons who had already had the smallpox—a proportion infinitely greater than that in which the smallpox occurs a second time. But, although we are unable to state the exact proportion of the vaccinated persons, in whom revaccination succeeded at the rate of 34 per cent., who would otherwise have acquired smallpox on exposure, yet experience has shown that this might have been considerable: whereas, among the many thousands who have undergone revaccination in Prussia and Wurtemberg, an example of the occurrence of smallpox has only here and there been observed. Moreover, in the case of an epidemic breaking out, it has been found, in various localities, that immediate revaccination has *arrested its course*—individuals in whom the operation proved successful and those in whom it failed equally resisting the disease."

The interval after which the system becomes impressible for the second time to the vaccine impression seems, from these reports, to be somewhere about seven years. But now, having answered this, there is another question: Is there any relation between the susceptibility to vaccine virus and to variolous virus? In other words, is a person who is susceptible to a second impression of vaccine necessarily susceptible to smallpox? Dr. C. thinks that, reasoning from analogy, we must believe that there is, and that a person who is susceptible to the vaccine impression, is also to that of variola. It is true that we cannot fix this by actual experiment very readily, for the system of the individual is more susceptible at certain times than it is at others. We vaccinate a child repeatedly without making any impression upon it. Two months afterwards, upon another introduction of the virus, it "takes." So with variola, persons at one time exposed to the contagion, are unaffected; at another, a much slighter exposure gives them variola. Dr. C. himself was exposed for six months on board a ship where the disease was prevailing for that length of time, and men died of it in its most aggravated form. He did not, however, become affected until the disease was leaving—his case being the last but one, and that one was a brother officer, who had at the beginning of the

epidemic, six months before, assiduously nursed a case both night and day, without taking the disease himself. These facts should make us chary of deducing from the immunity of a person at any particular time that he is *well* protected; and with the facts exhibited in the Prussian reports, numbering cases by hundreds of thousands, if the numerical system is worth anything, we ought surely to feel that it is our duty to encourage revaccination, and at periods not longer, as a general average, than seven years. Nor should there be any discredit to vaccination or detriment to the glory and greatness of Jenner's discovery. The prophylactic is just as good as he demonstrated it to be, it only has to be administered several times, at long intervals in the lifetime, instead of one dose sufficing.

Dr. DURKEE said that if the reports respecting revaccination in Prussia are reliable, it is quite certain that the vaccine element in that country possesses different qualities from those universally attributed to it in this country. We must either assume that these reports are not entitled to credibility, or else admit that the fame of Jenner rests upon the sand.

Or what is probably nearer the truth, we must suppose that there is some defect in the mode of conducting the business of vaccination among the Prussian troops. If a physician among us were to state that he had vaccinated in a thorough manner five hundred persons within the last five years, and that, of these, one in every four had been successfully vaccinated a second time, should we not look upon the statement as a very strange one, judging of it in the light of our own experience? Should we not suspect some radical error in his method of treatment?

A second attempt at vaccination often results in producing a pustule in the arm of a person who has already had true vaccinia; but this passes through its several stages more rapidly than in the genuine disease, and its aspect from beginning to end is wholly unlike true kine-pox. Nor does it produce constitutional symptoms; or if it does, they are of the most insignificant and equivocal character. The whole affair, in all its features, is a mere counterfeit.

Dr. D. was confident that the experience of the thousands of physicians who have been in practice in the United States twenty or thirty years, would sustain the opinion that one genuine vaccination is sufficient for life. This is the rule. To this there are exceptions, but these exceptions are rare compared with the millions of our population that are protected. There is good reason to believe that the great majority of physicians feel a personal responsibility in all instances of the vaccine disease which they undertake to produce; and if the vaccination is properly conducted, it is not probable that more than one out of a thousand persons, thus carefully treated, ever suffers from varioloid or smallpox. Take the city of Boston: Since the existence of the municipal law which requires that every child shall have a certificate of vaccination from a respectable physician, before it can be admitted into any of our public schools, probably more than 50,000 children have had kine-pox, and it is truth to say that physicians and citizens generally consider these children safe. The law has its origin in this idea of safety, which is the result of many years' experience relative to the prophylactic power conferred upon the system by the vaccine disease. This confidence need not be shaken by any tidings that come to us from the Prussian army or from any other quarter.

Dr. GRAY had repeatedly revaccinated patients at their request, but in no instance had a good vesicle been produced. He was, therefore, led by his own experience to believe the proceeding useless, and to oppose a repetition of the operation, on the ground that it would create an unnecessary alarm in the

community. It remains to be proved by its advocates that a second or even a twentieth vaccination renders an individual more secure than the first. There are instances to show that revaccination, in case it be necessary, should be repeated at very short periods, indefinitely. Two cases of varioloid had occurred in his practice during the past summer, in children under five years of age, one of whom had been properly vaccinated within a year. It was advised by many that revaccination should be practised wherever individuals were liable to exposure. Without denying the wisdom of such advice, he would merely state that, with a single exception, no case under his care of variola or varioloid during the last twenty years could be traced by the patient to any known cause. From the circumstances above mentioned, Dr. G. was led to express the opinion to his patients that one vaccination only was advisable, and since the alternative is a frequently-repeated operation, rendering the community constantly anxious about this formidable disease, he must be thoroughly convinced of his error before he would do differently.

Reference having been made to the distinction between variola and varioloid, Dr. STRONG said that he would consider all those cases which "stop short of the full developments of smallpox, as *varioloid*;" in these, the disease "fades away," as it were. In respect to contagion, and its power over individuals, Dr. S. supposes the amount of exposure will be the criterion of frequency in taking the disease; he himself had varioloid in two weeks from the time of inhaling, accidentally, the breath of a person ill with smallpox.

[In defining variola, authors give the preference to the division into discrete, confluent, and modified smallpox; the latter is the disease "modified" by previous variola in the patient, or by vaccination, and of course answers to the term *varioloid*, correctly speaking. Dr. Watson prefers to say "modified smallpox." *Varioloïde:* Synonymie—*Variole modifiée; varicelle pustuleuse ombiliquée.* (Grisolle.)—SEC'Y.]

In regard to the Prussian army statistics, members seemed to differ, widely, in opinion. Dr. STORER was surprised to find that they were brought forward, as worthy of any confidence in fixing a result of any sort—he had supposed that they were, long since, pronounced unreliable.

Dr. PERRY, referring to the researches of the late Dr. FISHER, remarked that Dr. F. had given his opinion in favour of revaccination, and that he had expressed confidence in the Prussian statistics.

Dr. C. E. WARE thought the Prussian experiments were only "half made;" they only prove a susceptibility to revaccination, not to the taking of smallpox or of varioloid. Dr. W. revaccinates, if asked to do so.

In reference to the soreness of the entire arm, frequently following revaccination, Dr. PUTNAM remarked, that it is quite often ascribable to irritation from the clothing over the punctures. If desired, he revaccinates; he mentioned the case of a lady who had had smallpox by inoculation, in her youth; on revaccination, a well-formed vaccine vesicle was produced.

Dr. CABOT related a similar case.

Dr. INCHES inquired of Dr. COALE, whether, in the Prussian army, those who were revaccinated for the third time, would be considered fully protected?

Dr. COALE supposed not, *with certainty*, but they assuredly were protected in great measure.

Dr. SHATTUCK, in an instance of the occurrence of varioloid, had revaccinated certain members of the family; in one person there was an abortive pustule; varioloid was subsequently declared in that individual. No other case in the family.

Dr. PUTNAM asked if any of the members present could offer any valid objection to revaccination?

There was no reply. The only objections previously made were the occasional sore arms, and alarm of the community. If solicited by their patients, they would doubtless not *refuse* to revaccinate.

["Some have recommended the repetition of vaccination at intervals of a few years. This is a wise precaution, justified by experience, and, if adopted, should be performed a second time at about ten years of age, or from this to the age of puberty." (*Hooper's Vade Mecum*, p. 251.) Dr. WATSON (*Pract. Physic*) says: "There seems reason for suspecting that the protecting influence of cowpox diminishes by lapse of time, and at length wears out;" * * revaccination at a distant period reproduces, in a greater or less degree, its primary effects. He mentions the case of a father and son revaccinated; the boy somewhat affected, the father not at all. "It remains to be determined," he adds, "whether all those who are susceptible of some impression from a second vaccination, are liable to be affected by the contagion of smallpox; and whether a repetition of the operation of engrafting the cowpox, renews, or adds to, their security against smallpox. At any rate, the practice of revaccination is a safe and advisable precaution.—*Op. cit.* p. 739, vol. ii. English edition.

During a session of the *Société de Médecine Pratique* at Paris, March 2, 1854, the following facts and opinions were given, as stated in the *Gazette des Hopitaux Civils et Militaires*, of April 29, 1854:—

"*Variola; Influence of Vaccination.*—M. DUPERTHUIS had seen, during the winter just preceding the above dates, 57 variolous patients; of these, 35 were from 3 months to 18 years, old; 22 were from 30 to 60 years; between the ages of 18 and 30 years there was not a single case; 30 of the 57 had been vaccinated; these were among the oldest; there were 2 deaths—ill-fed and feeble infants were the subjects; one patient, a man of 32, was quite delirious."

M. Duperthuis states that he revaccinated several persons who had already been submitted to the operation; he did not obtain any characteristic vesicles (*boutons*).

M. GUERSANT remarked: "That the statistics of M. Duperthuis, although upon a small scale, as to numbers, were important and instructive. Thus, variola had occurred 35 times in patients under 18 years of age, many of whom had never been vaccinated. In this there is nothing surprising, except that, since certain of this number were inoculated, the result would prove that the vaccine disease does not infallibly (*sûrement*) protect the individual up to the said age. On the other hand, as no case of variola was observed between the ages of 18 and 30 years, admitting the undeniable (*irrecusable*) influence of the vaccine inoculation, it would be shown that vaccination may protect from smallpox up to the 30th year; and, finally, there having been 22 cases of variola in persons between 30 and 60 years of age, it would follow that after the 30th year, it is requisite to be revaccinated."

M. Guersant alluded to the greater exactitude which would have been attained, had M. Duperthuis noted down precisely the number of persons in each series, who were, or not, vaccinated.

M. MASSON said "he could not entertain the opinion generally received, that variola is rare in vaccinated children. He had revaccinated 17 individuals, 5 of whom presented a perfect vaccine vesicle," (*les caractères d'un bon vaccin.*)

M. THORE remarked "that we must not believe an infant is vaccinated, merely because it has been submitted to the operation of the inoculation of the vaccine virus. From the fact that the eruption is not watched throughout its successive developments, it often happens that abortive pustules (*faux*

boutons) are mistaken for good ones. Moreover, on the other hand, very marked varioloid may be pronounced to be genuine variola, and every one is aware that, so far as contagion is concerned, a great distinction is to be established between the two affections."

M. Masson "could not adopt (*rejeter*) the principle that vaccine protects from variola until the 30th, or even to the 20th year. He had seen a lady, 19 years of age, attacked, after her accouchement, by confluent smallpox, while upon her arm there were large scars of vaccination."

M. Guersant replied "that vaccination has both its partisans and its opposers (*detracteurs*); but the facts given by M. Duperthuis support the opinions of the former, and are in favour of revaccination."

The above discussion has been slightly abbreviated; the facts and opinions given seem of sufficient value to authorize the extracts made.

The opinion of GRISOLLE is given in favour of revaccination. He remarks as follows: "This important question is not yet settled. Nevertheless, very numerous official documents, collected particularly in Denmark, Sweden, Germany, Prussia, and England, and certain of which have been analyzed by two distinguished physicians, MM. Dezeimeris and Hardy, in the 2d volume of the journal *L'Experience*, prove that variola is very frequent in individuals who have been vaccinated. This is an undeniable fact, and has, indeed, been long known." M. Grisolle quotes M. Serres, as stating that, within a few years, more than one-third of the persons reported diseased with variola have been the vaccinated; and the proportion increases yearly. Again, "variola (Grisolle) which is rare within 9 years from vaccination, is very frequent (*sévit surtout*) in persons who have been vaccinated for 10, 15, or 20 years. This authorizes us to believe that the vaccine virus only temporarily protects from variola, and that it is proper to revaccinate. It may be objected that (which is indisputable) after 35 years of age, the susceptibility of contracting smallpox becomes very feeble, indeed nearly null, which proves, according to Serres, that the presumed diminution of the preservative of the vaccine virus is not the sole cause of vaccinated persons being affected with variola." The author insists on the susceptibility of young subjects to the contracting of variola, &c.; from this he would apparently deduce an argument for more frequent repetition of vaccination in youth, and so proportionately to diminish the operations with the lessening necessity. Furthermore, M. Grisolle adds: "Revaccination is generally practised in the countries of northern Europe, and is beginning to be adopted in France. By the aid of revaccination, applied generally, variola has been almost completely extinguished in the Prussian army, and in the kingdom of Wurtemberg."—*Pathologie Interne*, vol. i. p. 105.—SECRETARY.]

Tænia.—Dr. STORER referred to a case. A middle-aged woman, after suffering from an irregular condition of the bowels for several months, voided three feet of this parasite, and after the administration of an active cathartic, two feet more were passed. Half an ounce of kousso was administered, in infusion, without producing the slightest effect.

Dr. S. referred to this case, particularly, to call the attention of the members of the Society to the unusual number of similar cases which have been reported during the *last* year.

Dr. Storer asked if cases of tænia are not comparatively rare in this country? From the infrequent reports of cases, hitherto, such would seem to be the fact.

Dr. ABBOT saw a case, the last week, and advised pumpkin seeds as a remedy.

Dr. PUTNAM mentioned a case in which these seeds were successful.

[Prof. PATERSON (*Philadelphia Medical Examiner*) states that, in 1852, he reported a case of radical cure of tænia by the use of pumpkin seed emulsion, after oil of turpentine and even kousso had signally failed. The fixed oil from the seeds is the anthelmintic principle, and has been employed with the happiest effects in doses of ℥ss. twice in the day, followed by an ounce of castor-oil.—SECRETARY.]

Tubular Pregnancy.—The specimen, with a history of the case, was sent by Dr. T. F. OAKES. The patient was an unmarried woman, twenty-five years of age. Complained of occasional nausea and vomiting, and of pain in the region of the uterus, for a week or ten days before the fatal attack. On the 16th ult. she arose in the morning better than usual. At 11 A. M., while doing some light work in the yard, she was suddenly attacked with violent pain in the abdomen, faintness, and nausea. With some difficulty she was got upon a bed, and Dr. O. saw her half an hour afterwards. She was then so exhausted as to be unable to answer questions. Countenance pale and haggard; eyes sunken, and the whole surface cold and moist; pulseless; nausea excessive, with retching and occasional vomiting of bilious fluid. Complained of great pain in lower part of abdomen, with tenderness on pressure. Stimulents and opium were used, but she died in about four and a half hours from the time of the attack.

About fourteen pints of coagulated blood were found in the peritoneal cavity. The pelvic organs were removed, and after a partial examination, were handed to Dr. Jackson, by whom they were shown to the Society. The uterus is enlarged, measuring four and a quarter inches in length, now that it is cut open, and it is changed in structure as is usual in the gravid state. The decidua is remarkably well-developed throughout the fundus and body, but ends, as usual, at the commencement of the cervix; the only peculiarity noticed being the absence of the characteristic punctated appearance.

The existence of a decidua, in the case of tubular pregnancy, has often been questioned, as is well known. The left Fallopian tube, which contained the ovum, was of about the usual size for some distance from the uterus, and then very suddenly dilated into a thin, firm, membranous cyst, which must have been nearly as large as the fist, there being nothing like a decidua upon its inner surface. The ovum was sufficiently well-developed, and the fœtus was not far from three months old, the spine measuring two and one-eighth inches in length; cord seven and three-fourth inches long. The fimbriated extremity could nowhere be traced, and neither could the seat of perforation in the tube be found. These cases usually terminate, as is well known, in the rupture of the tube and hemorrhage into the peritoneal cavity. Dr. J. remarked that in all of the cases he has seen, excepting one, the hemorrhage had occurred at the sixth or eighth week; the period in the present case being unusually late. The ovary was closely connected with the dilated tube, and contained a well-marked corpus luteum, $1 \times \frac{1}{2}$ inch. It should have been stated that a pretty distinct puncture was found upon the inner surface of the body of the uterus, as if some mechanical attempt had been made to procure abortion.

Upon the other side was a fine specimen of dropsy of the Fallopian tube; a thin, firm, membranous cyst, of a rounded form, nearly, or quite the size of the fist, and filled with clear, yellowish serum. Upon the surface of this cyst, and intimately connected with it, the tube was distinctly seen when inflated, being considerably dilated until it was just about to open into the sac, where it suddenly contracted so as not to admit the smallest probe. The opening, however, was seen upon the inner surface; and the fact of its being free was

proved by inflation. The inner surface of the sac had the appearance of a serous membrane. Fimbriated extremity quite obliterated. Ovary spread out over the sac, and intimately adherent, though distinct.

Powerful Effects from small Quantities of Opium and certain of its Preparations—Ingestion of five grains of Opium—Excessive Narcotism, &c.—Cases—Remarks—Table of Cases reported to the Society.—Dr. MORLAND read the following account:—

A gentleman 76 years old, of spare habit and of much muscular activity, though somewhat infirm, and who had been suffering from loss of appetite, costiveness, &c., depending on slight derangement of the biliary organs, took five grains, accurately weighed, of solid opium, in pilular form, it having been mistaken for blue-pill mass; both substances lying together. The pill was swallowed on going to bed, at about 10 o'clock P. M., April 7, 1854, and the stomach was quite empty, nothing but fluids having been taken for the two previous days. It being noticed that Mr. —— had not risen at his usual hour, a friend went to his room and found him fast asleep and breathing heavily, with some stertor; not supposing this of any consequence, he was left, and was afterwards visited twice, at about hour intervals, with the same result; it now being considered unusual, the same friend suddenly thought of the possibility of mistake, the evening before, in the medicine taken, and, examining the two substances alluded to, found, to his dismay, that opium had been used instead of the blue mass. A few minutes after this discovery, I came to the house for the purpose of visiting another patient, and was desired to see Mr. —— immediately. On entering his chamber, found him lying on his left side, breathing with an occasional snore; he was bathed in perspiration, which, increased by warm coverings, had soaked his night-flannel and shirt, completely—had wetted the sheets and pillow-case, and even the ticking of the bed. He was immediately shaken, and attempts made to rouse him; the skin of the face was of a dark-red colour; he spoke indistinctly, and said that he would get up, that he had had a very good night, and he wished to know why he was shaken, &c.; between these expressions, he would sink off towards sleep again; finding him unable to rise, his linen and flannel were changed, and his body well dried with a towel, and he was then *taken up* and dressed by myself and an assistant, apparently almost insensible. He was next *walked about* and taken out into the air, his legs bending and dragging, and he sinking upon one assistant, if the other for a moment left him. Although it was supposed that the opium was wholly absorbed, having been taken under circumstances so entirely favourable to its complete effect, emetic action was freely induced by means of salt and water, and mustard and water (no more powerful emetics being at hand); emesis seemed more efficient than anything to cause an approach to waking; strong coffee was freely administered, with some effect; the fumes of strong water of ammonia were allowed to enter his nostrils, &c. &c.; continual efforts through the day were required to keep him awake—the eyes closing immediately on ceasing to engage his attention, and heavy sleep with stertorous respiration occurring whenever he was left quiet. At times, the lethargic state seemed to have a sudden access, and scarcely by shaking or by pinching, could he be aroused; often, he did not feel severe pinching.

Taking into consideration the previously constipated state of the bowels, and the additional reason for purgative action, injections of soap and water were given, freely (five in all; Oss at a time); also, hydrarg. chloridi mit. et pulv. jalapæ, āā grs. x; pulv. ipecacuanhæ, grs. v, in one powder; and, after some

hours interval, there being no operation, one and a half drops of croton oil were given, in pill of crumb of bread; in from ½ to ¾ of an hour, a very moderate discharge took place. The urine was suppressed throughout the day, although efforts were made by the patient to pass it, occasionally, from a sensation as if of need. At the fecal evacuation, however, it passed freely. About this time there came on a troublesome itching of the arms and chest. Pulse, about 60 through the day; towards evening, 68 to 70; feeble. The dusky-red of the skin of the face, seen in the morning, changed to *extreme* pallor in the forenoon, and so remained; pupils of the eyes not remarkably affected; perhaps a slight contraction. The soporose state left the patient *quite suddenly*, within a few minutes even—efforts to keep him awake being no longer found necessary. The night was restless, with snatches of sleep, only. The effects of the dose were evident for many days; headache, obstinate constipation, unwillingness to move, and weakness, persisted; there was also complete anorexia. Patient is now, April 20, as well as usual.

Although the quantity of opium absolutely requisite to suspend vital action must always vary with circumstances, and often, especially, by the habits of the individual, even the quantity taken in this case might well enough have been fatal, under the existing conditions. CHRISTISON has recorded a case in which four and a half grains, combined with nine grains of camphor, were fatal in nine hours. Under other circumstances, doubtless, five grains would hardly have caused such very marked effects as have been above related. The prolonged effect of so little, comparatively—the excessive resistance to purgative agents and the long-continued *after* influences (not certainly very usual after recovery from the immediate effects), may seem of sufficient importance to warrant a report of the case.

Dr. Christison opportunely draws attention to the importance of knowing, approximatively, the *smallest fatal dose;* for, says he, "in consequence of the dread entertained by many unprofessional persons of this drug, it is currently believed to be much more active than it is in reality, and instances of natural death have been consequently imputed to medicinal doses, taken fortuitously, a short time before (*op. cit.* p. 657). The recording of such powerful effects as are occasionally observed from small quantities even, of opium and its preparations, may also be of service by inducing greater caution in the use thereof, without medical advice and prescription; it may tend to prevent the keeping of such potent remedies in places where mistakes are liable to be made, and where reckless use of the drug is possible.

GRISOLLE states that "he saw narcotism induced in a lady by the ingestion of three centigrammes of opium;" the *centigramme* being about one-sixth of a grain, this would give only *half a grain* for the quantity taken. The same author adds that twenty centigrammes (grs. 3¼) have sometimes killed *adults*, and that one or two drops of laudanum may cause the death of a newborn infant. (*Path. Int.* vol. i. p. 827.) Laudanum has been said to become stronger by *being long kept;* Dr. Bigelow, Sen., declares this to be incorrect, at least when dropped without shaking;—if the sediment be stirred, a *very slight* increase of power is possible. ORFILA (*Dict. de Médecine*, p. 261) says: "I have seen all the symptoms of an extremely severe (*pénible*) narcotism experienced by two young ladies, after the application of opium plaster to the temples." He does not state the proportion of opium used in preparing the plaster; certainly the amount absorbed must be, comparatively, very small. TAYLOR (*Medical Jurisprudence*) gives a case as occurring in 1838, of a man, aged 45 years, who died from the effects of ten grains of solid opium which he had swallowed. Also, another instance, in which eight grains

proved fatal to a woman, 38 years of age; the quantity, moreover, was taken in two doses. Interval not given.

Many authors refer to the fact of very powerful and alarming effects upon young children by exceedingly small doses. Orfila cites the statement of SUNDELING, "that the smallest doses (*les moindres doses*) act with activity in infants; the works of medical authors abound (*sont remplis*) with facts which support this opinion." (*Dict. de Médecine*, en 30 vols. p. 259.) Members of this Society have, in former years, reported very marked instances. Dr. HALE related the production of narcotism in a child one month old, by one-twentieth of a grain of acetate of morphia. The same gentleman also stated the occurrence of "alarming" narcotism in a child, from taking one drop of laudanum; and said, moreover, that he had witnessed in another child the same symptoms, after the use of "cough mixtures" containing minute quantities of opium, in some form; and again, by "cough lozenges." Dr. H., from observation of many such instances, concluded that opiates have a greater effect upon children, in proportion to the quantity taken, than does any other medicine. (Vide *Records of Society*, 1843.) With a view of presenting the results of several cases of poisoning by preparations of opium, so many as it is possible to collect by search of the volumes of past records, will be grouped in a tabular form below; among them are other examples of strong narcotic action from minute doses, and especially in infantile cases. In consequence of the loss of one volume of the Society's *Records*, any cases therein cannot be cited; one or two are not tabulated, from want of precise data. It is believed that all but two or three are given.

While referring to cases observed in children, one published by Dr. EDWARD SMITH, of London, in the April number of the *Association Medical Journal* (London), may be mentioned. In this instance, one-twentieth of a grain of opium proved fatal to an infant six days old, in eighteen hours after the administration of the dose. Syrup of opium, of the French Pharmacopœia, was the preparation; three-fourths of a grain of crude opium to the ounce of fluid. Artificial respiration was most efficient towards keeping up the action of the heart, which latter was quickened or slackened, according to the use of this means. The usual symptoms of poisoning by opium were induced in about half an hour, coma closing the scene.

In 1838, at a meeting of the Society, Dr. G. C. SHATTUCK, Jr., while referring to the liability of children to be even killed by very small quantities of opium, remarked that, in adults, the habit of drinking ardent spirits may have some effect in diminishing the susceptibility to the action of opium. Dr. S. at that time asked Dr. Bigelow, Sen., what he would consider the minimum fatal dose of opium or laudanum?

Dr. BIGELOW replied that, according to the Edinburgh Dispensatory, it is about four grains of opium, or one hundred to one hundred and twenty drops of laudanum. He added, that he had previously stated to the Society the occurrence of several cases in which an ounce of laudanum had been taken with impunity. (*Records*, 1838.)

An important question, in all cases, is the *quality* of the preparation, and as laudanum is perhaps most commonly used for suicidal purposes, we observe, oftenest, variations in the effects of large and of small doses; idiosyncrasy in individuals is a more rare, but doubtless not an infrequent element in the wide difference of the action of the drug.

A case was some years since reported to the Society by Dr. CHARLES E. WARE, in which ℥ij of laudanum were taken by an adult female, entire recovery ensuing; she was not in the least narcotized, but, on the contrary, was

highly excited; there was spontaneous vomiting. She bought the tincture at two different druggists. It is very possible that an inferior quality of laudanum was dispensed; indeed, with a superior kind, her escape would have been extraordinary.

In the case which forms the basis of this paper, attention may be directed to the profuse *diaphoresis*. This is remarked by Christison in a fatal case examined by him. "The sheets," he says, "were completely soaked, to a considerable distance around the body." This effect of opium has been lately referred to in the Society, when it was administered, in the ordinary doses, frequently repeated, in inflammatory affections; diaphoresis being often obtained very readily, although some have supposed its use would tend, constantly, to constrict the skin.

Subsequent to the reading of the above case and remarks, Dr. PUTNAM mentioned an instance in which five drops of laudanum caused decided and alarming narcotism in an infant seven or eight months old. He also stated that thirty drops of laudanum thrown into the rectum of an adult patient, who had had profuse diarrhœa, caused powerful and even dangerous narcotism. In consequence of such occasional accidents, he is very cautious in the use of opiate injections in cases where there has been violent purgation or diarrhœa.

Dr. PARKMAN said he had known forty drops, given by injection into the rectum, to cause narcotism.

Dr. SHATTUCK, in this connection, reported the following case. The supervention of narcotism, in this instance, from a small amount of morphia, is the more remarkable, because the existing pain would have been supposed capable of counteracting much of the effect of the medicine:—

W. E. C., ætat. 15 years, entered Hospital May 21, 1854.

Patient reports that, on the 12th inst., being quite warm from exertion, he drank freely of cold iced water, and on the same day ate five cents worth of mixed candy. Soon afterwards, was taken with abdominal pain and bilious vomiting.

At entrance, complains of abdominal pain and tenderness. Lies on right side, with knees slightly drawn up. Abdomen not full; the countenance expressive of languor and discomfort; cheeks of natural colour; skin dry; pulse 92, small, quick, and regular; tongue with dirty yellowish coat. Bilious vomiting. Pain somewhat relieved by sinapism.

May 22. Pain in abdomen continues; expectorated ℥ij aerated serum, with small masses of yellow mucus floating in it, and streaks and specks of blood; occasional vomiting. ℞. Sol. morph. sulph. ℥j, every four hours; blister, 2×3, to epigastrium.

23*d*. Reports better; lying on right side, as yesterday; very languid and sleepy; has not vomited since last night; skin rather cool. No dejection. Large enema of infusion of flaxseed; sol. acaciæ gum., for drink.

24*th*. Actively delirious through the day, and very restless through day and night; no perception of light or sound; two free dejections from enema; no vomiting; passed urine involuntarily twice in the night. Stupid most of time since 6 A. M., and can now scarcely be aroused; pupils contracted, but no suffusion of eyes; no heat of head; pulse 90, more full than yesterday; respirations from four to seven per minute; skin temperate. Has taken medicine regularly until 3 A. M.; none since; swallows with difficulty. Omit morphia; blister, 2½×3 to back of neck.

26*th*. Lying still on right side; occasionally groaning; some restlessness; skin natural; pulse small, sufficiently strong, about 100; tongue moist, with whitish coat. Has taken food with relish. Abdomen quite tense, hard; resonance dull inferiorly; catheter introduced, and ℥xii pale urine withdrawn.

June 1. Has been doing well; taken light food. Has vesicular and exanthematous eruption on abdomen.

3*d*. Walked out.

6*th*. Diarrhœa produced by eating oranges and a plate of baked beans, in market; slept pretty well in the night; abdominal pain; five or six dejections. Hop fomentation.

9*th*. No tenderness of epigastrium, but soreness complained of in the hypogaster. Vomited yesterday, P. M.; matter ejected containing some bile, which, he says, is generally the case. ℞. Pil. hydr. gr. iij, repeated in six hours.

17*th*. Has been gradually improving to present time.

We have here a case of gastritis, from a large quantity of cold water suddenly introduced into the stomach when the patient was heated; pain and vomiting continuing eleven days, and subsiding under the influence of morphia, of which enough was accidentally taken to produce narcotism; but out of this state the patient came, without any other remedy than a blister to the back of the neck. The convalescence was long, the patient being a delicate boy; and though his diet in the Hospital was carefully regulated, he was imprudent elsewhere, and such causes had something to do with the protracted convalescence. The quantity of morphia taken was about a grain, in divided doses, during an interval of forty hours.

Death from Laudanum (℥j) *in three-quarters of an hour.*—The following account of a case, which occurred in 1849, and was then reported, verbally only, to the Society, has been furnished, at the request of the Secretary, by Dr. G. H. Lyman.—Mrs. ——; age 52 years. Intemperate; has been drinking more or less for three weeks. An ounce of laudanum was taken by the patient, with suicidal intent. Dr. L. saw her in fifteen minutes. She said she had taken the laudanum for diarrhœa; was able to walk about with little or no assistance; begged to be allowed to lie down.

Nothing peculiar in pulse, as to force or frequency; pupils natural; external temperature natural; no evidence of sopor. Dr. L. gave two emetics of sulph. zinc.; could get no stomach-pump. No effect produced by emetics.

In thirty-five minutes after taking the laudanum, she began very suddenly to lose her pulse and muscular power; slight spasms were observed; the lips became livid; there was spasmodic dropping of the lower jaw; the extremities were cold, and, in ten minutes, she was unmistakably dead. The moment her pulse began to fail, frictions and sinapisms were resorted to, and preparation was made for the douche and artificial respiration; but she died before they could be instituted.

She had taken a hearty breakfast about three hours before. Her husband says her habit of drinking was induced by taking great quantities of tinctures (that of opium among others), after an attack of traumatic tetanus, ten years previous to her death.

Three quarters of an hour only elapsed from the ingestion of the laudanum to her death. Beck reports a case fatal in two hours. Christison says, seven to twelve hours is the average duration.

A case is reported in the *Boston Med. and Surg. Journal*, vol. xi. p. 285, fatal in two and a half hours. Christison also remarks that intoxication retards the action of opium.

[A case, fatal *in three quarters of an hour*, was reported shortly after the above, by Dr. Coale.—Laudanum, the preparation; amount taken, not known.—*Records*, 1849. Secretary.]

Cases of Overdose of Various Preparations of Opium reported to the Society since August, 1838. Chief Effects, Results, &c. &c.

Preparation.	Quantity.	How taken.	Subjects.	Results.	Effects observed.	Means used, &c.	Year.	Reporter.
Laudanum	℥j	Suicidal intent	Adult male	Recovery	Spontaneous vomiting; nothing serious.	None recorded.	1838	Dr. Otis.
do.	"2 tablespoonfuls"	By mistake	Adult female	do.	No ill effects.	None recorded.	1838	" Hayward, Sen.
do.	7 drops		Child, 22 mos.	do.	"Severe" narcotism.	Powerful emetics.	1838	" Fisher.
do.	2 drops	In a mixture, by a drunken mother	Child, 6 to 8 weeks	do.	Stupor; lividity of face through 1 day.	None mentioned.	1838	" O. W. Holmes.
do.	℥j	Suicidal intent	Adult male	Death	No particulars given.	None mentioned.	1841	" Channing.
do.	℥iss		Adult female	Recovery	Coma; slow respiration; insensibility; cold surface.	Powerful emetics; injections; no effect for hours: hyson tea.	1841	" Storer.
do.	1 drop		Infant	do.	"Alarming" narcotism.	No means recorded.	1843	" Hale.
do.	℥iij	Probable suicidal intent	Adult male	Death	Dying when first visited.	No means used.—*Post-mortem examination.* Congestion of stomach and small intestines; dark purple color of heart.	1843	" Coale.
do.	℥ss	Not stated	Adult female	Recovery	Spontaneous vomiting.	Sulphate of zinc ℈j. Free emesis.	1845	" J. B. S. Jackson.
do.	℥j	Not stated	Adult; sex not stated	do.	No particulars given.		1845	" Hale.
do.	5 drops	Injection into rectum	Child, 18 mos.	Death in 6 hrs.	Powerful action; symptoms alarming from first.	Had had cholera infantum.	1845	" J. B. S. Jackson.
do.	℥j	Suicidal intent	Adult female	Death in ¾ of an hour	After 35 minutes, failure of pulse and of muscular power; slight spasms; lividity of lips; spasmodic dropping of lower jaw; cold extremities.	Emetics; sulphate of zinc; no stomach-pump could be procured; no emesis produced by zinc twice given; frictions, sinapisms, &c.	1849	" Lyman.
do.	℥j	Suicidal intent	Adult female	Recovery	Thorough narcotism; livid hue; insensible to impressions; no taste.	Ipecacuanha, ℈j—no effect; sulphate of zinc, grs lx, in 3 doses—no emesis; mustard given with some effect; "walking about;" infusion of tobacco, in large quantity, in coffee; powerful emesis followed, with gradual recovery.	1851	" Strong.
do.	℥ss	Not recorded	Adult male	do.	No particulars given; "effects" of opium mentioned; subsequent mild pneumonia.	Injection of alkaline and acid mixtures, alternately into the stomach—no emesis; sulphate of zinc—free emesis.	1853	" C. E. Ware.
do.	5 drops	Not recorded	Child, 7 to 8 months	do.	Alarming narcotism.	No means recorded.	1854	" Putnam.
Laudanum	30 drops	By injection into rectum	Adult male	Recovery	Dangerous narcotism.	No means recorded.	1854	Dr. Putnam.

Laudanum	30 drops	By injection into rectum	Adult male	Recovery	Dangerous narcotism.	No means recorded.	1854	Dr. Putnam.
do.	40 drops	By injection into rectum	Adult; sex not stated	do.	Narcotism.	No means recorded.	1854	" Parkman.
Opium in substance	90 grains	"Taken in a freak"	Adult female	do.	Slow respiration; rapid, full, throbbing pulse; coma; strongly contracted pupils; effects noticed in about an hour; seen by physician in 3 hours.	Bleeding; emetics; washing out of stomach by stomach-pump; walking patient about; pinching, slapping, &c.; strong coffee.	1845	" J. B. S. Jackson.
do.	2 grains	By mistake	Child, 6 mos.	do.	Slow and difficult respiration; spasms; convulsions.	Washing out of stomach; injecting coffee and brandy into stomach; wetting of head with cold water; sinapisms to extremities; termination by violent purging.	1845	" Coale.
do.	3ij	Suicidal intent	Adult female	do.	Found on the street, vomiting; great excitation; pulse frequent, full; pupils contracted strongly; complete stupor, even after emetics; pulse subsequently slow; respiration very slow; stupor passed off, and excitement recurred: then stupor, from which patient was easily aroused.	Emetics; 10 grs. of sulphate of zinc with 3ss of ipecacuanha; dose repeated in a short time, with addition of 20 grs. sulphate of zinc and 3ss ipecacuanha; no emesis produced; patient placed in the erect posture, and the stomach washed out, then 1½ pints of strong coffee thrown into it; patient began to rally in 20 minutes after coffee; walking patient about, brushing surface of body, &c.	1845	" J. B. S. Jackson.
do.	¼ grs., with ¼ grs. extract of belladonna[1]	Accidentally	Child, 9 years	do.	Deep sleep; spontaneous vomiting, from which great exhaustion.	None recorded; the drugs taken after a full meal.	1853	" Coale.
do.	5 grains	By mistake	Adult male	do.	Excessive narcotism.	Emetics; forced exercise; coffee, &c. &c.	1854	" Morland.
Paregoric	"1 teaspoonful"		Child, 6 weeks	do.	Stupor.	Emetic; no emesis caused.	1845	" Channing.
do.	3j	Suicidal intent	Adult female	do.	Coma; insensibility to impressions.	Stimulants; emesis by feather in throat; walked about; cold affusion.	1854	" Coale.
Acetate of morphia	1-20th of a grain	In cough mixture; repeated small doses	Child, 1 mo.	do.	Deep sleep; could not be aroused to take the breast.	No means recorded.	1843	" Hale.
Sulphate of morphia	1 gr. in divided doses through 40 hours	Medicinally; in solution	Boy, 15 years	do.	Narcotism.	On omission of morphia, effects passed off.	1854	" Shattuck.

[1] Dr. Anderson (*Monthly Journal*, April, 1854) declares belladonna *remedial* in poisoning by opium;—this instance would seem confirmatory.

[The *smallest* amount recorded in the above table is *one drop* of tincture of opium; its effect, "alarming narcotism;" patient, an infant; recovery.

The *largest* amount recorded is ninety grains of opium; coma; recovery.

The smallest amount recorded as *fatal*, is five drops, *by injection* into the rectum; patient a child, eighteen months old.

Smallest amount fatal, *when swallowed*, an ounce of laudanum; two instances; adults; in one, death in three quarters of an hour.

Preparation of opium most frequently taken, *the tincture;* 17 times. Opium, in substance, 5 times; paregoric, twice; acetate of morphia, once; sulphate of morphia, once.

Total of cases reported, 26; male adults, 6; female adults, 8; children, 9. One boy of 15 years. Two, sex not stated.

Suicidal intent, 6 times; accidental, 4 times. Administered medicinally, 6 times; taken "*in a freak*," once; intent not recorded, 8 times; in a mixture, carelessly, by a drunken mother, once.

Most efficient means: Emesis; stomach-pump; forced motion; cold affusion; external stimulation; internal stimulants.

Owing to the lack of record, it has not been possible to state the intervals elapsing between the ingestion of the poison and the beginning of its action, and the termination of the same; neither can the time intervening between the swallowing of the drug and the visit of the attending physician, be ascertained, in a sufficient number of cases to make its record here of any avail. —W. W. M.]

Colloid Tumour of the Neck—Several Operations—Frequent Recurrence—Death.—Dr. PARKMAN, exhibited a specimen, with daguerreotype views, and gave the following history of the case. A single woman, 39 years of age, a school teacher by profession, consulted Dr. P. in March, 1852, for a tumour the size of a goose egg, in the left supra-clavicular region of the neck. This was subcutaneous, smooth, round, without lobulations, elastic and fluctuating, as if a cyst with thick fluid contents, usually unattended with pain, but at the menstrual periods there was a marked increase of sensibility. The diagnosis being uncertain, it was decided to cut down upon the tumour, if a cyst, discharge the contents, and to proceed as might be indicated. This being done, there was discharged a thick tenacious, gelatinous, amber-coloured matter, so remarkably glutinous and adhesive, that it could be drawn into filaments two feet in length, resembling, very much, thick mucus, and filling the interstices of a sponge used to wipe it up in the same manner. This was followed by a more consistent substance, slightly grayish in colour, and with faint traces of structure. The finger passed into the cavity showed this to be circumscribed by an almost imperceptible cyst; and the various muscles, as the edge of the sterno-cleido-mastoideus, and the trapezius, could be readily detected. It seemed as if this substance had been deposited directly in the cellular tissue, occupying the space between these muscles. With a view of exciting inflammation and thus closing the cavity, a seton was passed through it. Such was not the result, however; but the cavity soon became filled with tumours growing from its walls. These were soft, painless on pressure, and easily broken down without hemorrhage or any ill effects consequent upon the manifestations. It seemed as if the tumours were composed of the gelatinous matter already described, deposited in a very fine cellular tissue. Under these circumstances, in May, the whole disease was removed by a clean dissection, exposing the muscles and fascia of the lower portion of the neck. The tumour thus removed presented the characteristics already described. Soft,

friable, glutinous, and held together so slightly by a thin areolar tissue, that its removal was impossible in any way but as an amorphous mass. Cicatrization went on rapidly, and in a very short time the patient was attending to her duties. She continued to enjoy perfect health, until March, 1853, when she called Dr. Parkman's attention to a small tumour outside of the original cicatrix, the size of a hazel-nut. There had been pain in the part previous to its appearance, three weeks since, but now there was no abnormal sensation. This tumour was immediately removed, a simple incision only being required. It was situated in the subcutaneous cellular tissue, and presented exactly the same semifluid, gelatinous, glutinous substance as the large tumour. In April a similar tumour, in May two similar, and in August one similar were removed. The appearance of these was always preceded by pain, which ceased when they were developed. They were all outside of, and without any relation to the cicatrix resulting from the original operation, and could in no case be regarded as the growth of any part of the first tumour, which might accidentally have been left.

The wounds all healed rapidly, the patient being hardly confined to the house. The general health during this time remained perfectly good.

In November, attention was again called to another reappearance of the disease. This time, however, the patient being somewhat discouraged, had allowed considerable progress to be made without mentioning its presence. The disease had reappeared as usual, under sound skin, but with its growth it had insinuated itself beneath the cicatrix of the first operation, and now occupied the whole of the supra-clavicular space. The health remained perfectly good. The complete removal of this mass was again advised, consented to, and performed. This time the dissection was much more extensive, exposing the whole superficial anatomy of the supra-clavicular space. The recovery was, however, rapid. The disease removed presented exactly the same characters as before.

January 1, 1854, attention was again called to another tumour, the size of a hazel-nut, which with similar antecedents to all the others, had appeared below the clavicle, over the coracoid process of the scapula. It resembled exactly all the others, and could as easily have been removed. It was decided, however, to wait one month, and an appointment was made for the first of February. On the 20th of January, however, Dr. Parkman was sent for, and found the patient convinced that there was to be a new development of the disease, from the peculiar pain which she experienced over the lower half of the left sterno-cleido-mastoid muscle. An examination detected, in a space of about three inches square, a certain degree of fulness; so slight, however, that it would not have been observed by one not fortified by past experience in the case. A few days, however, were sufficient to remove all doubts on the subject. Within a week a tumour developed itself of the size of a female closed hand. The impossibility of any further operative influence was now evident.

The further progress of the case may be detailed in a few words:—

This tumour having already shown such a tendency to rapid growth, continued to increase, and finally attained a size fully equal to the patient's head, having for attachment the whole side of the neck, and hanging pendulous over the chest. At several points the skin became ruptured from the distension, and the mass unfolded itself through the openings, and several large sloughs were thrown off. The substance of the tumour as thus shown was soft, friable, and could be torn with the fingers, without much hemorrhage, and with

no pain to the patient, presenting always portions of the soft gelatinous matter with the more concrete texture, which has been described. With its advance, however, the tumour became comparatively more solid and firm, though without any actual change of structure.

With the growth of this tumour, the general health began to give way, and death finally occurred in June, about five months after the rapid development first described, and about three years from the commencement of the disease.

The *autopsy* showed extreme emaciation of the whole body. The tumour, as already described, was cut away from its bed, and it was then seen how the disease, originating in the cellular tissue, had gradually attacked the muscles in the vicinity. But a faint trace of the sterno-cleido-mastoideus muscle could be detected, and all the others were more or less involved. The softness of its texture, identical with that already described, prevented any preservation of its form. There was no other appearance of the disease save a deposit the size of an orange, on the edge of the opposite breast. The viscera of the abdomen and chest were perfectly healthy, save some œdema of the lungs, the symptoms of which had been observed a few days before death.

The points of interest in this case will be perceived to be the limitation of the disease, with but one exception, to a single region of the body; the rapid cicatrization of the wounds; the preservation of the general health until the last attack, and the very rapid growth of the disease—so rapid as to be almost perceptible from day to day.

Subjoined is a note from Dr. SHAW, giving the result of several microscopic examinations, made upon the tumours after their removal, at various times; as also upon portions torn from the surface of the tumour, when *in situ*, as has been described.

"My first examination of the tumour from the region of the clavicle, was in *March*, 1853, about a year after its first removal. The tumour was then made up of cells and free nuclei, with but a trace of fibres, and these very fine and pale. The cells were such as are generally found in well-marked cancer, except that they were somewhat smaller, and many of them, instead of presenting the globular or circular form, were elongated or otherwise distorted, having the appearance, so to speak, as if they had run wild. They did not, however, have the appearance of the fusiform fibro-plastic cell. The cells measured, on an average, 0.017 millimetre, and the nucleus 0.012 millimetre, the mean diameter of the typical cancer-cell being from 0.020 millimetre to 0.025 millimetre, and that of the nucleus from 0.010 millimetre to 0.015 millimetre. Though the cell walls were small, the nuclei were of the average size, generally oval, but sometimes globular, with dark and well-marked contour, and nearly filling the cell. Many of the nucleoli were large, highly refracting, and having the appearance of an oil-globule, whilst others were smaller.

"In *April*, the tumour presented the same appearances under the microscope. Many of the cells were elongated and distorted, as before, and their walls pale; yet the nuclei were very large compared with the size of the cells, and the nucleoli large and highly refractive. As before, the cells were a trifle smaller than usual. Small capillaries were observed, admitting the passage of only one or two blood-globules, and these were compressed and elongated so as to have an oval form.

"*November*, 1853. The cells were perhaps a trifle larger than before; the nuclei still large when compared with the size of the cell, with dark contour, and the nucleoli well-marked and oily-like. The nuclei were not merely larger and more prominent than those of the fibro-plastic cell by measurement, but they nearly filled the globular cells, or were nearly as large as the transverse diameter of the elongated cells. There were as before many free nuclei.

"*February*, 1854. At this examination, the cells would not pass so well for cancer-cells as at the previous observations. They varied from the normal form

more than ever, being less regular in size, and in many the nuclei were indistinct, or wanting. Some were found with large nuclei, but wanting nucleoli, and in many instances the nuclei themselves were small.

"*March*, 1854. Last examination. The cells and nuclei at this time were still further from the typical form of cancer. The cells were quite small, but now generally globular, and very pale; the nuclei nearly filled the cells, but were pale, and below the average dimensions; the nucleoli were mere points, or were absent. The cells were few, the free nuclei predominating. None of the cells were fusiform in this or the previous observations, nor did the cells or nuclei correspond to the type of the fibro-plastic elements.

"If any value is to be placed upon our present knowledge of cancer and fibro-plastic elements, it would seem that this growth should be termed cancer. At the commencement, its minute structure had all the appearance of cancer, except that the cells were somewhat smaller than the average (which is not an uncommon circumstance), and were considerably distorted in form. But the nuclei and nucleoli, during the earlier stages of the disease, were typical. They differed from the fibro-plastic elements both in actual and relative dimensions. The nuclei had the dark contour, and the nucleoli the refractive power which are wanting in the fibro-plastic nuclei and nucleoli. Why the minute structure became changed during the progress of the disease, of course it would be impossible to say. Possibly, from the superficial situation of the tumours, and their frequent removal, the cells had an opportunity of easy, rapid, and unrestrained development, such as they do not possess in the ordinary situations and forms of cancer.

"The peculiar character and history of this case may be some apology for an unsatisfactory microscopic examination; and it cannot be expected that the microscope will determine the nature of doubtful growths, until the minute structure of well-known morbid growths shall be better understood."

Syringe for the Injection of Melted Lard into the Vagina, in Parturition.—Dr. COALE exhibited a syringe which he had contrived, and found very useful for throwing warm lard into the vagina, in cases of dryness of that passage in protracted labours, particularly where the head had been pressing some time on a resisting perineum. It consisted of an elastic nozzle some three and a half inches long, to which is attached an India-rubber tube two feet long, having an ivory socket at the end. An India-rubber bottle with an ivory nozzle fitting accurately into this socket, completes the simple apparatus. The elastic nozzle is introduced, directed by the forefinger. The ivory socket remains outside the bedclothes. The nurse having melted the lard and filled the bottle, introduces its nozzle and makes the injection, during which the elastic nozzle in the vagina can be moved about to distribute the fluid. Dr. C. has found that the relief given in this way to the severe pain of tension, and sense of burning, is more than he would have supposed, whilst the labour was in several instances evidently greatly expedited, both by the mechanical lubrication of the lard, and by its softening the parts, and making them yield more readily.

June 26. *Pearl Gaiter-Button removed from the Left Nostril of a Child Eighteen Months Old.*—Dr. MORLAND showed this, and stated the difficulty he had experienced in extracting it, and which doubtless was in some measure owing to the smoothness, hardness, and shape of the foreign body. A polished disk of the size of this button is far less easily seized and withdrawn, or pried out, than is a bean or any such object. The eye of the button was broken, and, as was seen after its removal, presented a sharp point, which must have wounded the mucous membrane of the nostrils during the extraction, and caused some bleeding. A bent probe, a scoop, and the ordinary small-sized forceps were

unavailingly employed, the button seeming to glide and escape every effort at seizure. A pair of very delicate curved eye-forceps were finally used with complete success at the first effort. They were inserted *closed*, and employed as a lever, prying the button from behind forwards and outwards. It escaped with a *leap*, as it were, going to the distance of several feet. Dr. M. added, that surgeons recommend the use of a small *curette*, or *scoop*, or a *bent probe* (Druitt, Fergusson), or else that, failing extraction, the foreign body "be pushed back into the throat." Removal by the anterior nares is surely very desirable. As to the *scoop*, in one or two instances, the size usually furnished in the pocket-case has been found too large by Dr. M. Certainly, in infants or young children, who are most frequently our patients in these instances, such is likely to be often the case. In several instances mentioned to him, the operators were obliged to resort to very delicate (though strong) instruments; and in one to those contained in a case of eye instruments.

The child above referred to, introduced the button at about 7 o'clock P.M., June 23, and it could, by account, be readily seen at first; but in the hurried and injudicious efforts made by the nurse and others to remove it, they pushed it completely out of sight, and it could with difficulty be reached by instruments, when the child was brought to Dr. M. at 10 o'clock P. M. The child is small, and the features delicate. The nasal apertures quite small. Button nearly half an inch in width, and one-eighth of an inch thick.

Carcinoma of Liver.—Case reported by Dr. STORER. Margaret M——, widow, aged 50, entered Hospital, June 11, 1854. Was well until January last. At that time, noticed an enlargement in right side of abdomen; appetite became impaired, and she experienced slight bilious vomiting, and more or less drowsiness. For last three months has been unable to work, and for some weeks past has kept in bed all the time. For the last ten days has rapidly failed in flesh and strength; except on one occasion, during this period, has not taken any nourishment except wine-whey. Much emaciation; skin deeply tinged with bile; tongue with a dirty, whitish coat; bowels free. On examination of abdomen, a hard, resisting body is observed in right hypochondrium, extending from the edge of the short ribs behind, to within an inch and a half of umbilicus, and outwards and downwards, to inguinal region. Remainder of abdomen tympanitic throughout, with the exception of a small portion just below epigastrium, which is very tender upon pressure, and slightly emphysematous.

Advised tumour to be bathed night and morning with tinct. iodine, and patient to drink freely of the infusion of wild-cherry bark, and to have wine-whey *ad libitum*.

13*th*. Complains of great pain in abdominal tumour. An effusion noticed on the right of tumour, occupying a small circumscribed space. Copious bilious dejections.

14*th*. Much less pain in tumour upon pressure.

15*th*. Constant disposition to vomit; matter ejected, a dark-coloured, bilious fluid. Complains of great prostration. Skin dry and husky. Pulse 120. ℞. Mist. calcis carbonatis ℥ii; tinct. opii gtt. x. every two hours until relief.

16*th*. Vomiting continues; hands cold and damp; pulse 120. Died at 6 P. M.

The following notes by Dr. ABBOT, the Admitting Physician of the Hospital, who made the autopsy, are copied from the Hospital Records:—

Body greatly emaciated, of a bright, golden-yellow colour. Lungs healthy. Some slight, old adhesions between upper and middle lobes of right lung. Heart

healthy. Half an ounce of yellow serum in cavity of pericardium. Before opening abdomen, the tumour, recognized during life, was distinctly felt; also a decided fluctuation quite across abdomen. On opening abdomen, the stomach was found to be enormously dilated, extending across the front of abdominal viscera, and from ensiform cartilage to pubes. It contained a quart or more of greenish-black fluid, somewhat viscid. Previously, as much more escaped from the mouth in removing the body before opening it. Stomach very thin, pale, and suffused by cadaveric changes. A pint and a half of yellowish serum was found in the abdominal cavity. Intestines generally much injected, with here and there portions of slimy lymph on surface.

General contour of liver normal. In the substance of the anterior portion of right lobe a mass of firm carcinomatous disease of the size of a turkey's egg was imbedded, not, however, coming to the surface or affecting the outline thereof. A part of the organ free from disease. Colour generally presenting no unusual appearance. Under surface of that portion where the disease existed, adherent to a mass of carcinomatous disease somewhat larger than two fists, which extended to the left and backwards across vertebral column, to which it was firmly adherent, including the pancreas and gall-bladder, and adherent to the viscera in its vicinity, viz: the stomach and large intestines. The mass was, in general, of great firmness; portions of it, however, broke down easily under the finger. It was supposed that an abscess of three or four inches, in an irregular cavity, was broken into and discharged in securing the specimen. Other organs not examined.

Dr. J. B. S. JACKSON gave the following particulars relative to the *post-mortem* appearances:—

An encephaloid mass, not far from the size of two fists, existed in the region of the gall-bladder, and matted together all the neighbouring parts; the liver itself being of about the usual size, and somewhat, though not very extensively, diseased. Upon cutting through this mass, the gall-bladder, which could not before be found, was exposed, and in its cavity was about a dessert-spoonful of biliary calculi; the parietes were dissected, but it did not appear that the carcinomatous deposit commenced in them.

Female Catheters;—preferable pattern.—Dr. PUTNAM remarked that he was lately requested, by a medical gentleman, to employ the catheter for a member of his family. The instrument used was perforated with holes in the usual way, and gave the patient no uneasiness.

A short time afterwards, the gentleman informed Dr. P. that he had introduced the catheter himself, but that, in attempting to withdraw it, he met with considerable resistance, and, on persisting, the pain, which had already been severe, increased to an amount he had rarely witnessed in surgical operations.

The catheter he had used, had, instead of holes, a slit on each side about one-tenth of an inch in width and half an inch in length.

It was suggested that the mucous membrane might have got sucked into these slits and thereby caused the difficulty; but, lest it should be due to peculiar irritability of the bladder, or to some other cause, it was decided to etherize the patient rather than submit her to a repetition of the pain. This was done once only, and a catheter with the common holes was afterwards used without difficulty.

It happened on the following day that another medical gentleman consulted Dr. P. in regard to a patient in whom a similar difficulty attended the use of the female catheter. This also was made with slits, and on substituting the other, all trouble ceased.

Dr. Putnam almost invariably had used a catheter with slits, in the male subject, and had never met a similar embarrassment.

Remedies in Rheumatism—Antimony and Opium—Lemon-Juice.—Dr. HOMANS referred to his former use of antimony and opium, combined, in rheumatism; when free diaphoresis took place, he had found them efficient at cure.

Dr. H. had given lemon-juice, very freely, to one patient, lately; there had been both gout and rheumatism. Age of patient, 35 years; quantity taken, the contents of one-half a dozen lemons the first day (which was the fourth day of the disease); second day, eight or ten lemons consumed; and, before finishing, the larger part of a box. The attack appeared to be diminished in intensity and shortened in duration, according to the usual average of continuance. There was subsequent excessive derangement of the digestive organs. Dr. H. had never seen so much disorder follow the employment of the lemon-juice—for weeks there was complete anorexia.

July 10. *Aneurism of the Aortic Arch compressing the Trachea, without Physical Sign of Aneurism.*—Dr. H. J. BIGELOW exhibited the specimen, and presented the following case, drawn up by a relative of the patient:—

The deceased was 37 years of age, of a remarkably vigorous constitution, free from any constitutional ailment, and since his youth has not experienced an illness, of a day, sufficient to detain him from his daily business until his final one. About two years since he complained slightly of an affection in his throat, which he supposed was bronchial, as at times he felt a tickling sensation, and at times also expectorated considerably. He did not then complain of difficulty in breathing. He remained in this general state for perhaps a year and more, sometimes so little troubled that he did not speak of it for weeks, but upon taking cold, he complained of his former difficulty. This bronchial affection, as he supposed it to be, became aggravated at intervals until some time in March of this year, when, as he afterwards stated, he ran about a quarter of a mile through the rain to arrive in season to take an omnibus, and thus save a long journey on foot. From this he dated his first real difficulty in breathing. Shortly after this he discovered a slight whistling noise, which he could make voluntarily, but which was not constant, without such effort. This noise increased rapidly, and he began, in April, to find that too much exercise was an additional aggravation to it. Early in May he was examined by Dr. Bigelow, with reference to the cause of this singular noise, and his obstruction in breathing; these examinations suggested several causes, and among others that of aneurism as most probable.

At this time a very little exertion "put him out of breath;" he was obliged to walk very slowly, and the effort of going up stairs was so great that it required a considerable time to recover his tone. He returned to Boston, and in a few days the exertion of walking became too much for him, and he then kept in the house, gradually restricting himself in his movements. From June 18, he was mostly confined to a sofa, where his occupation was to arrange and rearrange his pillows to gain some comfortable breathing position. For a number of days previous to this, he said that he could sensibly feel the obstruction as the air, in its difficult expirations, glided over it. Until within ten days of his death he insisted on going up and down one flight of stairs, but it was a very slow and tedious exercise. He was carried up the last time, and after being laid upon the bed, he lost his breath and sprang for the open window; failing here, he ran through the house to the window in

the front of the house, where, after two and three-quarter hours of most superhuman exertion, he recovered sufficiently to rise from his knees, and was placed upon a couch which had been prepared for him at this window. In this effort his breathing was sometimes gasping, hoarsely and loudly, and sometimes produced quite a shrill whistle. He was never removed from this window—all the doors and windows on this story were kept constantly open to provide air, and his efforts to raise began then to strangulate him, so that he was liable to spring up choking, at any moment. The second day after taking his last position, he strangulated and was held nearly three hours, seemingly, in the agonies of death, gradually growing weaker and worse, until, having passed through every stage, he fell lifeless into the arms of those who supported him; his pulse had then stopped; there was no perceptible beating of the heart; his eyes, and indeed everything, betokened death. He was then placed upon the couch, and in this situation he remained perhaps three minutes, when suddenly his eyes closed, his lungs became inflated, and in two or three minutes he spoke aloud, in a tone more natural than he had employed for months. He then said that he breathed about as well as ever—felt no obstruction nor any uncomfortable sensation excepting such weakness as made it even impossible to move his arm. He thus breathed freely six or eight hours, when, upon putting the ear to his chest, the same sounds as before became audible. This extreme difficulty came on again, and he lived to pass through seven spasms like the one described, varying in intensity, but diminishing as his strength passed away. During the first spasm his expectoration began to be mingled with blood, and ever after and from that time, he complained of a new kind of obstruction above the place of the other, which, from his raising, appeared to be caused by clotted blood. All day, July 4 and 5, his strength was so utterly gone that he pushed down the obstructions from the outside with his finger, gradually breathing shorter and shorter until evening, when he expired.

The specimen presented a dilatation of the arch of the aorta, apparently ensuing upon atheromatous degeneration, about three inches in diameter, adherent to the trachea, where it crossed it just above its bifurcation, compressing it, and opening into it by an orifice of the size of a dime, which, however, was most effectually plugged by a slender, thin wall of lymph—the result of inflammation, and which had alone hurried death by hemorrhage.

Dr. Bigelow had repeatedly ausculted the patient, as had also several gentlemen, among whom was Dr. Bowditch, and no abnormal sound whatever, connected with the circulation, could be detected—neither impulse, thrill, nor murmur. The obstructed trachea alone yielded a noisy respiration, a little louder in one lung, its maximum near the supra-sternal fossa.

Dr. B. remarked that similar cases of absence of evidence of thoracic aneurism are not very rare. He thought that thoracic compression of the trachea, gradually increasing, might, in default of evidence of other disease, be attributed to aneurism as its most probable cause.

Urinary Calculus formed upon a Leather Shoestring.—Dr. Brown, of Bangor, Maine, exhibited the specimen, and reported the case. The patient was a man, æt. 27, and grossly addicted to onanism. Fifteen months ago he passed the string into the urethra after he had gone to bed, and went to sleep without removing it; in the morning it had disappeared. One or two months afterwards, urinary symptoms appeared; and these became so urgent, that he was obliged to give up work last autumn, and for some time past has kept his bed. About a week ago the calculus was removed by Dr. Rich, of Bangor,

assisted by Dr. Brown; the lateral operation of lithotomy was performed, and the patient has done well since.

The calculus is of a regular, oval form, somewhat flattened, and measures nearly two inches in length. The chemical composition, according to the analysis of Mr. J. C. White, student of medicine, is: "Phosphate and carbonate of lime—the latter being slight—with a slight trace of double phosphate of ammonia and magnesia. No uric acid nor urates in the portions examined." One extremity of the calculus having been broken away in the removal, the foreign body has been fully exposed.

Prof. MUSSEY, of Cincinnati, who was present at the meeting, mentioned a similar case in which he had operated, about two years since. The patient was a young man who had introduced (in accordance with medical advice, as he stated) a piece of cord into the urethra, for the purpose of allaying irritation. While introducing it, an unexpected occurrence surprised him, and caused a suspension of the operation, during which time the cord disappeared.

Dr. Mussey saw the patient three months afterwards, and was led from the symptoms to suspect the presence of a calculus in the bladder; at that time, however, it was impossible to detect any stone on examination by the sound. Two months later, a calculus was at once distinctly felt, and the operation of lithotomy was performed. The calculus extracted resembles a petrified lumbricus. The nucleus was the cord introduced, as above mentioned.

Monstrosity. Anterior Parietes of the Abdomen Deficient—Peculiar Form of Spina Bifida.—The specimen was sent to the Society by Dr. R. M. HODGES, and exhibited by Dr. JACKSON. Labour occurred at the seventh month; the woman (Irish) having previously had a large family of children. Pains came on about noon, and she set out to walk a distance of between one and two miles to obtain a permit of entry into the Almshouse. On her way she felt something coming from the vagina, and this subsequently proved to be the intestine of the fœtus. Her case being urgent, she was sent to the Cholera Hospital, as being nearer than the almshouse, and there Dr. H. saw her at half-past three P. M. On examination, the intestines, liver, and the two elbows were found to present; the lower extremities, as it appeared afterwards, being bent entirely backwards; a hand was soon forced down, after which there came one pain and the fœtus was expelled, doubled up in a most remarkable and altogether reversed position. This was at 4 P. M. The child was quite fresh, though stillborn.

The anterior parietes of the abdomen are entirely wanting. The liver is a very prominent object, and somewhat irregularly developed; gall-bladder and ducts traced. Spleen of usual size. A considerable portion of the stomach and intestine had been torn away before Dr. H. saw the woman. The rectum opens freely upon the serous surface, but near to the line of the integument. The urinary bladder consists of two cavities, sufficiently and about equally developed; these communicating freely towards what would be the neck, and the one from which the remains of the urachus arises, communicates almost throughout with the rectum. The right kidney was very prominent, and in the condition of hydronephrosis, being as large or larger than the stomach of the fœtus would have been if distended; presented the usual appearances upon the cut and upon the inner surfaces of a distended kidney, and was filled with fluid. The ureter was much enlarged, and towards the bladder ended in a cul-de-sac. Left kidney sufficiently normal; ureter accidentally cut off. Renal capsules normal. Upon each side of the

opening from the rectum, and at some little distance from it, though still upon the serous surface, was another opening of an oval form, and sufficiently large to admit a small director. The symmetry of these two openings, in regard to position, size, and form, was striking; and on passing in a probe, it was found to play about freely as in a large cavity. One of these cavities was then cut open, and was found to be a pretty well developed vagina. From this last there arose a uterus, having its os tincæ and arbor vitæ well marked, and sending off from its fundus, and towards the right side, a distinct Fallopian tube. The vagina and uterus upon the left side were not cut open, but externally the appearances upon the two sides corresponded precisely. There was upon each side, then, one-half of a uterus, with its tube, and one-half of a vagina, each being nearly or quite as largely developed as the entire organ is in a well-formed fœtus; the two uteri being quite prominent objects, situated far apart, near the margin of the integument, and directed upwards and outwards. The ovaries were not found. A trace of the external labia was seen on each side, and just where the integument ceased. The two pubic bones, as they may be called, were separated, as usual in cases of extroversion of the bladder, and to the extent of nearly or quite one and a half inches. The umbilical cord was inserted, as usual, upon the margin of the integument. Thoracic organs sufficiently well.

Spina bifida also existed. A sac, nearly or quite the size of two fists, hung off from the region of the sacrum, consisting of integument, lined by a serous membrane, and containing, by estimate, ten or twelve ounces of serum; opening from the spinal canal, prominently seen, as soon as the sac was laid open. Dr. J. remarked upon this form of spina bifida as having been observed here in some few other cases, and he thought it had not generally been described. The peculiarities in this case are in the situation over the sacrum, and in the extension over the tumour of a well-developed cutis; whereas in ordinary spina bifida the malformation is higher up, and the integument is deficient to the extent of the malformation. The large size, and the thinness of the sac, with the large collection of fluid, are also observed in the present variety; though in two cases, on the contrary, that have been observed here, the tumour resembled a female breast over the lower part of the child's back, and in one of them, which was dissected, the cavity within was quite small, and there was a large quantity of fat between it and the skin. The fœtus was otherwise well formed externally.

Varicose Arterial Tumour.—Dr. H. J. Bigelow presented the specimen, a tumour upon the forehead of a middle-aged female. It was of the size of a large hen's-egg, directly over the right eye, projecting upon the forehead, and dipping beneath the orbit; convoluted in its surface; of a brilliant red, pulsating heavily, and filling to its utmost capacity in about three seconds after the blood had been pressed out of it as from a sponge. It was painless, of about two years' duration, gradually increasing, and originally near the hair, from which situation it seemed to have gravitated to its present position. Numerous dilated arteries radiated from its circumference. At the operation, the larger trunks were secured by ligature, but the tumour continued to fill with blood until nearly the whole periphery had been included in successive ligatures. With a view to its radical extirpation, it was then rapidly excised within the ligatures, when the exposed surface bled obstinately, even after the use of the actual cautery. The wound rapidly healed. A wax injection of the tumour was shown, from which the skin had been dissected. In form, it resembled a solid bundle of common earthworms. The convoluted arteries

suddenly expanding and contracting, mutually to inosculate, were inextricably interwoven.

Dr. B. remarked, that a thesis, with plates, had been long ago written upon this rare disease by Breschet. He had himself seen it in two cases in the hand. In the case of a young girl, described by Dr. Townsend, an affected finger had been since amputated, and some of the dilated trunks in the hand and arm tied with great relief. In the other, the case of an old woman, one finger had been converted into a flexible, pulsating mass, the bones having undergone almost complete absorption.

July 24. *Membranous Croup in the Infant—Cases, &c.*—Dr. COTTING, Associate Member of the Society, showed the trachea of a child who died of *croup* at the age of fifteen months, and gave the following account of the case: Patient was taken on Tuesday P. M., July 18; passed a restless, uncomfortable night. The difficulty of breathing was the chief symptom, but was attributed to a "stuffed cold." During the next day, the breathing, though difficult, was not so distressing as the night previous. At dark, on Wednesday, the labour of respiration was so great that the parents became alarmed, and sought medical advice. They had not considered it croup, although they had had previous experience in the disease. Two years before, an older child (five years) had a severe attack, and, after five days of constant inhalation of steam, threw off the membrane and recovered. According to Dr. C.'s experience, very few cases of membranous croup are suspected to be croup, or to be of much moment, until the physician gives the startling announcement. Other forms of croup, so called, but misnamed, and of very little danger, generally from the onset cause great alarm.

The patient from whom the specimen was taken, died on Thursday, 20th, after an illness of about thirty-six hours. The croupy exudation covered the under side of the epiglottis, was more consistent over the glottis, and formed a complete membrane, which could be readily raised for about five lines below the chordæ vocales. The inner surface of the remainder of the trachea was of a bright red colour, diminishing in intensity from above downwards, but extending, quite visibly, below the bifurcation, and into many of the branches. There was some slight congestion of portions of the left lung, near the smaller bronchi. No other morbid changes noticeable.

The case is interesting on account of the youth of the patient. Some years since Dr. C. performed the operation of tracheotomy on a patient of less than seventeen months. The operation was performed on the following grounds, viz: the child's danger would not be increased; the parent strongly urged it, having in some way heard that it might afford an additional chance of relief; and lastly and chiefly, that if, as believed, the membrane, or exudation, progressing from above downwards is less liable to descend far, in inverse proportion to the age of the child, an opening made into the trachea low down would possibly go below the membrane, and offer a greater chance of success. The operation was unsuccessful. No *post-mortem* was allowed.

On relating this case to one of great experience in this disease, a doubt was thrown over the diagnosis on account of the youth of the patient, and on that account only. Admitting that no case can be proved to be membranous croup unless the membrane can be produced before or after death, and a *post mortem* in this case having been denied, there was no alternative but to wait till time and chance gave further opportunities of testing the question.

Two cases of recovery in children under one year, and one in a child of twenty months, have occurred to Dr. C. since the time alluded to. In these,

sufficient fragments of membrane were thrown off to prove the disease. In a family where one child had some months previously died of croup, he was called to a child of five years, in the last stages. While this child lay dead in the house, another, of six and a half years, was taken. During the sickness of this child, some slight symptoms of the disease were supposed to be detected by the mother in a pair of nursing twins. On examination, the throats of both these infants appeared to be covered with the croupy exudation. They seemed to be well; nursed well; and it was only occasionally in their crying and slight cough that any suspicious sounds or symptoms could be detected. If the disease had not been so fearfully present in the house, it would never have been suspected in these children. Some fragments thrown from their throats could not be distinguished from those from the oldest child, or those taken from the deceased. The cases of these two infants had a decided influence on Dr. C.'s willingness to operate in the instance before spoken of.

The specimen presented to the Society, proves unequivocally that true membranous croup has occurred at the early age of fifteen months. It is undoubtedly more frequent in older children; but when the diagnostic symptoms are present, the young age of the patient must not be too much relied upon.

Uterine Polypus.—Dr. J. B. S. JACKSON gave the account of the case and specimen. The tumour is nearly the size of two fists, arises extensively from the cervix, and hangs into the vagina, which is much distended; form rounded; structure indistinctly fibrous, a free incision having been made through the mass; some appearance of vascularity upon the external surface, and at depending part. Where the neck of the tumour passes through the os tincæ, the two are intimately adherent, though the outline of this last is distinctly enough seen. At one point, however, there is an opening through from the vagina into the cavity of the uterus, about large enough for the passage of a small bougie. The uterus itself, through which a free incision has been made from the fundus into the neck, appears to be in every way perfectly healthy. The patient has been under the care of Dr. Samuel Gregg for about fifteen months, and from him the following history was obtained: Her age was 36 years. She had miscarried five times before carrying a child to the full term, and each time with considerable hemorrhage. In January, 1850, she had her second and last child at the full term, preceded by dangerous hemorrhage, as the placenta was over the mouth of the womb. From this last confinement she recovered well, but did not nurse her child. She was afterwards troubled with leucorrhœa. In about four months she began to have attacks of colic and gastralgia, and soon afterwards she was treated for hemorrhage, which continued more or less at intervals until her death. Since Dr. G.'s attendance, she has been perfectly blanched, and mostly confined to her bed. She, however, retained her flesh and appetite, and for a few months last autumn and winter, when the hemorrhage was slight, she gained strength and colour. The tumour was spoken of by the physician who was previously in attendance, only a month or two before Dr. G. saw her; enlarged considerably afterwards; examinations always caused more or less hemorrhage; and it was examined only once with the speculum, at which time it appeared very much as it did after death. It was never painful nor tender to the touch; caused no sense of bearing-down, nor dysuria, and scarcely gave any consciousness of its presence. The catamenia were regular, and entirely without pain, though sometimes attended by hemorrhage; the most severe hemor-

rhages, however, occurred generally a week or two after the flow had ceased, from which circumstance Dr. G. concluded that they proceeded from the tumour. A serious leucorrhœa existed most of the time during Dr. G.'s attendance.

August 14. *Additional particulars of a Case formerly reported to the Society as "Ulceration of the Cæcum,"* (see this *Journal* for July, 1851, p. 65.) *—Death of the Patient recently—Post-Mortem Examination—Correction of the Diagnosis.*—Dr. STORER reported the following facts: In the summer of 1852, a year after the reference to her case to this Society, Miss L. was suddenly seized with severe pains in the back, where swellings in a few days appeared, followed by suppuration. In a short time an opening also appeared in the affected groin, through which pus was freely discharged. Her health became much impaired, and for a period her condition was extremely critical. During the year past, however, she has suffered much less, and has even remarked she "would almost have forgotten," to use her own words, "that she was not well." In April last, her feet began to swell, and about the last week in May, she became very feeble, and from that time has continued to fail. She died a few days since. Although no fecal matter has passed through the aperture in the groin, since the wound in March, 1851, air has continued to be thrown off at longer or shorter intervals, until her death.

The following *post-mortem* appearances, drawn up by Dr. CALVIN ELLIS, who kindly performed the autopsy, are very interesting, as well for the great malformations of the organs, as for the length of time the patient survived—for a portion of this time enjoying tolerable health.

A layer of fat nearly an inch in thickness in abdominal parietes.

The peritoneal cavity contained several ounces of serum. Much fat in the mesentery, meso-colon, and in those parts of the cellular tissue where it is usually found.

Several loops of the small intestine were firmly adherent to each other, to the parietes in the neighbourhood of Poupart's ligament, to the dense cellular tissue filling the pelvis, and to the sigmoid flexure of the colon.

Appendix cœci adherent to the lower half of right kidney, the cæcum lying below; both natural in their appearance. The last three or four inches of the small intestines were much contracted.

At a point about three feet from the pylorus, the small intestine was attached to the parietes, near the anterior superior spinous process of the ilium, and here a free communication existed between the carious surface of the bone and the intestinal canal, the opening in the latter being as large as a ten-cent piece; the mucous membrane immediately surrounding it was perfectly healthy.

The ilium in the neighbourhood of the superior and inferior spinous processes was in a porous, diseased state, breaking down easily under the probe, and was bathed in a dirty, very offensive pus. The exact limits of the disease could not be ascertained, but it communicated with the external openings in the groin.

Liver quite firmly, though rather loosely, attached to the diaphragm, and to some of the surrounding organs, by means of old, delicate, membranous bands. Its surface had a somewhat rough, cicatrized look. The cut surface presented the dark spots noticed in partial venous congestion.

Pelvic organs imbedded in a mass of dense cellular and adipose tissue. A phlebolite in vicinity of uterus.

Spine.—On making an incision over the lumbar vertebræ, the probe struck

what appeared to be denuded, but firm, bone. The muscles, however, were not discoloured as in diseases of the kind, and the opening immediately above this point (the fourth lumbar vertebra) communicated by a longitudinal canal with the diseased portion of the ilium.

Fracture of the Face, Clavicle, and Ribs.—Dr. COTTING, of Roxbury, reported the case. Dr. C. reminded the members that some years since, he related to the Society a case of recovery from a fracture of the entire face from the skull. (See this *Journal* for January, 1850, p. 71.)

A few days since, a boy, aged four and a quarter years, was run over by a carriage containing five persons. The soft parts of the face were lacerated on the right side, from the median line of the nose just above the cartilages, across the cheek to the lower edge of the zygomatic process of the malar bone, and from that point upwards to the outer angle of the orbit, completely denuding the bones of that side. The right superior maxillary bone, together with the malar, was completely broken from its attachments, and driven downwards and backwards for at least half an inch. The force of the blow appeared to have been received immediately below the orbit, a portion of the floor of which was separated from the maxillary bone.

Through the nose and the palate the separation took place at the median line; the nasal process standing off from its articulation, and the teeth and palate projecting into the mouth for the distance before mentioned. The zygomatic arch was broken down, and the articulations of the superior maxillary bone with the cranium, &c. severed.

The fractured portion was wedged in so firmly that, for some time, its reduction seemed almost impracticable. But after removing, with the saw and other instruments, many of the engaged fragments, it was successfully accomplished by the aid of a powerful trepanning lever.

The lower jaw, which was broken through the body on both sides and at the symphysis, was adjusted, and the fragments kept in place by tying the teeth together firmly with saddlers' silk.

The wounds were then closed with interrupted sutures, and cold-water dressings applied.

Over the left mammary process of the temporal bone there was a severe abrasion, where the head came in contact with the ground. The left clavicle was broken near its scapular extremity. There were also seven fractures of the ribs—the second, third, and fourth of the left side, anteriorly, near the junction with the cartilages, the fourth and fifth posteriorly; on the right side, the fourth, anteriorly and posteriorly. There was much effusion of blood into the neck and about the upper portion of the chest.

The boy was a patient of Dr. B. Mann, who rendered efficient assistance during the adjustment of the fractures.

Notwithstanding these severe injuries, the lad recovered his consciousness a few hours after the accident, and on the day following, sat up in bed, recognizing and speaking to some of his mates. His respiration, embarrassed from the first by the fractures of the ribs, grew more and more difficult. It was soon quite evident that his chief danger was from this source. Towards night of the second day he became more distressed. The respiration was partial, irregular, and painful. He remained conscious till about two or three hours before his death, which took place thirty hours after the injury.

The case is interesting in connection with the other, showing how great an injury the face and head may receive, without apparently involving the life of the sufferer.

[*Note.*—The Secretary takes this opportunity to correct an error in the report of the case of hydrophobia related by Dr. Cabot (*Extracts from the Records*, vol. ii. p. 47, *Am. Journ. Med. Sci.* April, 1854, p. 359). Dr. C. E. Ware is stated to have "inquired if there were any well-authenticated instances of recovery from the bite of a rabid animal?" Dr. Ware's question was: if there were such instances of recovery from *declared and positive hydrophobia*.]

ART. VII.—*Case of Paraplegia, caused by Concussion of the Spine. With Remarks.* By CHARLES A. LEE, M. D.

ACCIDENTS to the spinal cord may be considered, in the light of physiological experiments, throwing much light on its office and functions. Many such have been recorded. The case I am about to detail, has sufficient points of interest, perhaps, to render it worthy of permanent record.

In the spring of 1853, I was called to see Mr. Pope, of South Sodus, N. Y., a patient of Dr. Lewis Graves, from whom I learned the following particulars: Age 57, habits intemperate, health not very good for a short time previous to the injury. On the 28th of August, 1848, he fell from his wagon, striking his head and shoulders on the ground, the whole weight coming upon his head, which was thrown backward and under the body. On raising him up, he was found to be helpless, but wholly conscious. Dr. Graves found him, two hours afterwards, sitting upon the floor, a friend supporting him in an erect position. The arms and lower extremities were found wholly paralyzed, sensation and motion entirely destroyed; the knees were inclined to fall together, and were not easily separated; the hands were flexed upon the wrist; the thumbs and fingers turned in upon the palms of the hands; though he was unable to raise his hands to his head, he had some power to raise the arm and shoulders. On examination, there was found to be a partial dislocation of the sixth cervical vertebra, it being thrown partly to the left of the spinal column. The patient complained of tenderness on pressing over the parts injured. The urine had to be drawn off by a catheter for several days, after which the urine and feces passed involuntarily, though he was conscious when it was about to happen. The quantity of urine secreted was about the same as in health. The bowels were kept open for several weeks by catheters. The pulse was about fifty in a minute, and quite feeble; appetite generally good, and the food digested well for about four years. Since that time, the appetite has been poor, and digestion considerably impaired. Vomiting would often come on soon after eating; the feet and legs became anasarcous, and abdominal dropsy was also present. This condition existed for several years. There was sloughing of the cellular tissue of the nates and posterior part of the thighs, caused by pressure from sitting on a hard chair. About six months after the injury, he fell into the hands of a Thomsonian, and was treated by lobelia, emetics, and cathartics, during which, inflammation of the scrotum and penis took place, resulting in extensive sloughing of the parts. Dr. Graves, on being again called in, administered stimulants and tonics, under the influence of which the recuperative powers again rallied, and in the course

of two months the parts were healed. Since that time (1849), sores have occasionally appeared on those parts subjected to pressure, as from sitting or lying. The sense of feeling returned in some degree, about ten months after the injury, and gradually improved, while the power of motion was not improved. The treatment consisted chiefly in supporting the general strength by tonics, and suitable diet.

April 9, 1854. Present condition of patient: Sensation has returned, but the power of motion remains the same, being totally lost. The limbs are emaciated; both the flexor and extensor muscles of the hand and forearm are contracted, and have been permanently so since the injury. The surface is cold; pulse feeble, and ninety in a minute. Copious perspiration is excited by the least exertion. The patient is quite weak and feeble; is unable to sit up much; appetite poor; quantity of urine secreted unusually large, and discharged involuntarily; much pain in the back and limbs; is very restless, and sleeps but little. The bowels are moved once in forty-eight hours. Can move the shoulders and body by using great exertion. Several sores on the thighs and hips difficult to heal. Slight cough; tongue thickly furred and flabby; digestion very poor, and the power of the system gradually failing. The mind has remained all the time unimpaired, though occasionally feverish and irritable.

The following letter from Dr. Graves will complete the history of the case:—

"Dear Sir: I take the liberty of giving you a statement of the case of Mr. P., since my communication of April 9, last. The patient was then feeble, and the powers of life seemed failing pretty rapidly. He continued to fail from that time. Food and drink were ejected from the stomach without much effort, and soon after being swallowed. He complained of no nausea or sickness at the stomach, nor of any distress; still, food and drinks were not retained for about three weeks previous to his death. He was very much emaciated. Sloughing of the thighs had taken place, also the inside of the knees, by the contraction of the muscles, thus bringing them closely and permanently in contact. There was also involuntary twitching of the muscles of the legs, causing considerable motion and pain. The patient was very restless, requiring the attendants to turn and change his position frequently. The urine and feces were discharged involuntarily, as usual; respiration was short; pulseless at the wrist for several days before dissolution. The carotids beat feebly. The surface cold, and covered with a clammy sweat. Mind continued clear and undisturbed until death, which occurred on the 8th of May.

"*Autopsy.*—In the presence of Drs. Pearce, Peck, Mann, Trachout, and Reynolds, I made a *post-mortem* examination of the parts supposed to have been injured on the 8th. I carefully removed the five cervical and first dorsal vertebræ, and then laid them open longitudinally, so as to expose the spinal cord. The spinous process of the fourth, fifth, and sixth vertebræ were slightly depressed; the spinal cord was found to be firm and healthy from the second to the fifth vertebræ; within this last there was much softening and depression, the cord having the appearance of having been *bruised* or *cut off*. Below this point the coil was softened, and evidently shrunken or atrophied, with evidences of inflammation and engorgement of the bloodvessels. The conclusion, from all the appearances, was, that, at the time of the accident, a partial dislocation of the fourth, fifth, and sixth vertebræ had taken place, with depression of the spinous process, causing a *fracture* and *entire separation of the cord*, and giving rise to all the subsequent phenomena. I send you the specimen to examine for yourself, after which I would like to hear your views in regard to the case, and particularly with regard to the question how have the lower extremities been supplied with vitality, since the injury?

Truly yours, &c., Lewis Graves, M. D.

South Sodus, *May* 13, 1854."

Remarks.—On examining the specimen, I find that there is no evidence of there having been any *fracture* of the body of the vertebræ, although the spinous process of the second and third cervical appeared to be *carious.* On opening the thecæ of the cord, which was greatly atrophied, the spinal marrow was found softened and disorganized, while the dura and pia mater were slightly thickened, vascular, and closely adherent to the walls of the vertebral canal. Whether the spinal cord was entirely destroyed within the fifth or sixth vertebra, as stated by Dr. Graves, I could not certainly determine, as it was difficult to ascertain the extent of the degeneration of the nervous into fatty matter, though it was extensive.

Such a condition would be hardly consistent with the existence of sensation, which we have seen was restored, to some extent, during the latter years of life.

The case is an interesting one on many accounts; ramollissement and atrophy of the cord existed through the whole extent of the cervical vertebræ, while the caliber of the vertebral canal was in no point so much diminished as to interfere with the functions of the cord. Was it, originally, a case of concussion only, resulting in congestion and inflammation, and subsequent softening, or was it a case of fracture, the marks of which had disappeared? The latter can hardly be supposed possible; while there are cases on record which go to support the former conjecture. Abercrombie relates a case, where a man received a severe shock, by falling some ten feet and striking his back upon a pile of stones. *He was immediately paralyzed in the lower extremities,* and the urine and feces were passed involuntarily. The power of sensation remained, but at the end of a month, when admitted into the hospital, there remained complete loss of motion, and all the muscles of the affected parts were atrophied, and in a state of great flaccidity. There was deep-seated pain on pressure, in the region of the third, fourth, and fifth dorsal vertebræ; evacuations involuntary, breathing slow, appetite moderate, digestion good, mind quite entire, and pulse and other functions natural. He died about five weeks after the accident; trismus, convulsions, and tetanus having come on forty-eight hours previous to death. *Autopsy* disclosed no injury to any of the bones of the spine. The pia mater of the cord was highly vascular, especially at the upper part of the dorsal region; and there was extensive ramollissement of the body of the cord, chiefly in its anterior columns, and to such a degree, that in some places they were entirely diffluent. This extended to the upper part of the cord, and affected the corpora pyramidalia.

In the case, whose history has been given by Dr. Graves, it is, of course, impossible to determine how long the disorganization of the cord had existed, although there is good reason to believe that it commenced soon after the injury was received. The patient survived for nearly six years, after such a degree of injury, as to prevent, in a great measure, the influence of volition upon the lower extremities, or the transmission of sensation from them to the brain; and the usual phenomena were manifested, which are generally, if not

universally, present under such circumstances, viz: sloughing, slower repair and reproduction of parts, owing to impaired nutrition, involuntary twitchings, and lessened capacity of maintaining itself against the influence of external forces, involuntary twistings, &c. In this case also was strikingly exhibited what we so often find in atrophy and ramollissement of the brain, the breaking up of the nerve-fibres, and the production of abundant granule-cells or masses, and free-floating granules, similar to those commonly found in the granular or fatty degeneration of various cells of both normal and morbid origin, and called by Paget, "liquefactive degeneration."

Since the above was written, Dr. Graves, in reply to some queries addressed to him, has communicated the following particulars:—

"1. The portion of the cord, where it seemed to be bruised or cut, was between the fifth and sixth cervical vertebræ. When first examined, it was very distinctly felt by passing the finger from above downwards on the cord. There was a feeling, at the point mentioned, as though the cord had been cut off, and below this point the cord was found softened and diminished in size.

"2. His urine was ammoniacal and turbid.

"3. His bowels did not move involuntarily at all times. Cathartics acted kindly when given. The discharges were thin and watery.

"4. His mental faculties remained clear to the last, as in health. He had no tetanic spasms. There was pain on pressure over the injured part of the spine.

"5. His speech was slightly affected; voice weak, and had to exert himself to speak loud. There were but slight motions of the limbs, except involuntary twitchings.

"6. He had more power over the upper than the lower extremities.

"7. There was occasional vomiting, and at last both food and drink were rejected as soon as swallowed.

"8. At times he could not sleep, and two or three weeks before his death he became very restless, and slept but little; position had to be often changed.

"9. The sloughing upon the thighs was about four inches in diameter, and on the inside of the knees, about the size of a dollar, caused by their being drawn together; also a large spot on the back, where it came in contact with the chair-back.

"10. Medicines did not affect him as much as when in health; larger doses were required.

"11. There were no prominent contractions of the toes or of the fingers."

The above case requires no extended comments, as a careful examination detects no luxation or fracture—no diminution of the spinal canal; it furnishes an interesting example of lesion of function from violent concussion of the spinal cord; or, it is possible that, from the violent twisting and bending of the cervical vertebræ, the cord may have sustained serious injury, independent of luxation or fracture. It is generally supposed that, in cases of permanent paralysis of the inferior extremities, it must arise from one of the latter; this case, however, goes to prove that it may occur independent of such injury.

If we are to believe, as Dr. Graves states, that the spinal cord was cut and twisted off between the fifth and sixth cervical vertebræ, then we must con-

clude, as maintained by Todd and Bowman, that "the spinal cord shares, in some degree, in the functions of sensation and voluntary motion." These writers remark that "the recent discovery of the *amphioxus lanceolatus*, a small fish found in the Archipelago, makes it highly probable that voluntary motion and sensation may exist where there is a well-developed spinal cord, the anterior extremity of which tapers to a fine point, and is far from exhibiting the ordinary characteristics even of a brain so inferior in organization as that of fishes."[1]

Here there was a partial restoration of sensation and motion, with such a physical condition of the cord as would seem entirely inconsistent with the existence of such functions, viz: ramollissement, atrophy, and degeneration; and, according to Dr. Graves, an entire twisting off, or disappearance of the nervous matter of the cord, between the fifth and sixth vertebræ.

ART. VIII.—*Operation for Laceration of the Perineum.* By F. M. ROBERTSON, M. D., Lecturer on Obstetrics in the Charleston Summer Medical Institute. [With a wood-cut.]

IT is not my design to enter into a discussion of the manner in which this accident may occur, or to compare the merits of the various operative procedures proposed and advocated by different surgeons and obstetricians for the purpose of relief. In alluding to the slight reference to the accident by various writers on obstetrics, Fahnestock remarks:—

"This silence on so important a subject can only be ascribed to the general impression, that the accident is one of very rare occurrence. It may be, in the practice of judicious practitioners, for Dr. Dewees informed the writer, a few years since, that he had to contend with but one case, happening to himself, in his extensive practice, which he attributed to the patient attention he always bestowed on supporting the perineum during the expulsion of the child through the os externum; yet, such is the nature of things that, in some cases, with the best management, laceration of the perineum is unavoidable."—*Am. Journ. Med. Sciences*, N. S. vol. i. p. 99.

The author just quoted states that only one case of laceration occurred, under his management, in a practice of eighteen years. In a practice extending over a period—from 1829 to 1854—of twenty-five years, the accident has never occurred in any case, of either natural or instrumental labour, under the management of the writer.

During the past summer I was consulted in relation to a lady, aged about twenty-four years, who, I was informed, had suffered from this melancholy accident, while in labour with her first child, about a year since. She was attended by a midwife, and the occurrence was attributed to some mismanage-

[1] The Physiological Anatomy and Physiology of Man, Am. ed. p. 276.

ment on her part. She came to Charleston in December last, and placed herself under my care for the purpose of being operated on.

Upon examination, I found the laceration to extend from the posterior commissure of the vulva to the anus, implicating, to a considerable extent, the external sphincter; but neither the internal sphincter nor the rectovaginal septum. Nature had so far repaired the injury to the external sphincter, that, in conjunction with the internal, control over the fecal evacuations was preserved. The septum between the lower portion of the rectum and vagina was so thin that, by the advice of a physician whom she consulted, she daily used a cathartic calculated to produce liquid evacuations from the bowels, fearing that the discharge of accumulated hardened feces might complete its rupture. The lower extremity of the posterior columna of the vagina projected very much at the deep-seated margin of the laceration. The wound had healed irregularly on each side, and with considerable puckering and induration. The uterus had a constant tendency to prolapsus; the use of a supporter and pad had the effect of enlarging the external opening and increasing the difficulty. Altogether, the condition of the patient was one of great bodily suffering and extreme mental anguish; and she was anxious to undergo any operative procedure that would afford the possibility of relief. An early day was fixed for the operation.

After a detailed examination of the various modes proposed for the relief of this distressing condition, I determined to adopt that recommended by Bushe, and, without discussing the comparative merits of each method, I shall merely quote the following words from the work of this author.[1]

"To obviate the inconvenience which I have ascribed to the interrupted suture, I have devised a pin, which is represented in Plate VIII., Fig. 4. [See accompanying figures.] This instrument is as thick as that used for harelip, and consists of three parts. The first, which is made of silver, is from one and a half to two inches long, curved as represented in the plate, terminating at one end in a female screw, and at the other in a transverse shoulder, about a quarter of an inch long. The second is a triangular steel pin, exactly resembling that used for harelip, and screws into the extremity of the first portion. The third is made of silver, and resembles the transverse shoulder of the first portion, with this exception, that a small male screw passes vertically from its centre, so that it may be fixed into the first portion when the second is removed. This instrument is to be used in the following manner: the first and second portions being united, provided the tumefaction has nearly subsided, and granulations are formed"—[he is speaking of recent injuries"]—"the patient should be brought to the edge of the bed, her hips elevated, and her knees approximated and carried towards the chin. The parts being now cleansed, the needle ought to be dipped in oil and inserted into the left side of the perineum, a line more than half the breadth of its curve from the edge of the wound, and immediately above the verge of the anus. When it has passed vertically for a distance equal to two-thirds of the depth of its curve, its point should be projected transversely, so as to cross the bottom of the wound, and then carried outward through the other side of the perineum. This stage of the operation will be greatly facilitated, firstly, by pressing out the left labium during the transmission of the needle

[1] A Treatise on the Malformations, Injuries, and Diseases of the Rectum and Anus. Illustrated with plates. By George Bushe, M. D. &c. New York, 1837.

through the left portion of the perineum and the base of the wound; and, secondly, by steadying the right side of the perineum, with the extremity of the thumb placed immediately without the point through which we desire the needle may pass. When the puncture has been completed, the steel pin should be unscrewed and the third portion fixed in its stead. If it is thought advisable to insert a smaller pin higher up, it may be done, and then a thread should be twisted over their extremities, as in harelip. It may be prudent to place a light bolster of lint beneath the twisted ligature. This method of operating was first carried into effect in the almshouse of this city (New York) by my friend, Dr. Stevenson, who, not only on this, but on other occasions, afforded me opportunities of testing chirurgical innovations."—p. 82.

Having ascertained that the pins, as described above, could be constructed by Mr. Steinhardt, an excellent instrument-maker of this city, the 27th of December, 1853, at 12 M., was fixed on for the operation. The patient was directed to be kept upon a low diet for some days; the night previously the bowels to be freely opened by a cathartic, and some hours before the operation the rectum to be cleared by an enema. I regret that I was deprived of the valuable use of chloroform in this case. From the earnest solicitations of the patient and her friends—amounting, I may say, to a positive demand—I was compelled to operate with no other assistants than two ladies; and as my invariable rule, in surgical operations, is never to use chloroform unless its administration be confided to a skilful physician, I was compelled to dispense with its use. To the observance of this precaution, I attribute the fact that no untoward occurrence has ever taken place in my practice from the administration of anæsthetic agents.

The patient was placed across a bed, of such a height as to allow me to sit in a common chair in the proper position, fronting a strong light, with the hips drawn well over the edge of the bed, and the thighs and legs supported by the assistants, as in the operation for lithotomy. I commenced a short distance above the termination of the cicatrix on the left labium, and dissected, with the scalpel and forceps, a thin layer from without inwards a little beyond the mucous membrane, down to the bottom of the fissure, without detaching it. A similar dissection was made on the right side. The two layers were then seized together with the forceps, and removed, by a pair of scissors, from the anus to the projecting lower extremity of the posterior columna of the vagina. This procedure freshened every part of the laceration. Two small patulous processes—remains of the old wound—hanging from each labium on a level with the superior margin of the cicatrices, were also removed. The pins were then introduced as directed by Bushe; one near the anus, the other near the upper termination of dissection. The coaptation of the parts was perfect. No bolster of lint or compress was used. There was no eversion of the edges of the wound, as the crossing of the threads from one pin to the other kept them pressed down, and in perfect apposition. A catheter was introduced into the bladder and confined by tapes, her knees tied together, and, with the thighs flexed upon the pelvis, she was placed on her side, with directions, when she became fatigued, to be turned carefully to the other side, without separating the knees. The bowels were kept in a quiescent state by small doses of tinct. opii, repeated once or twice a day. The parts were daily washed, and a simple pledget of lint, saturated with a solution of the chloride of soda, lightly applied. She was confined to a fluid diet.

On the ninth day the union of the wound was complete, and the pins were removed. There was slight suppuration from one of the orifices of the upper pin, but, under the use of the solution of chloride of soda, this soon ceased.

On the sixteenth day the catheter was dispensed with, and, as the cicatrix appeared firm, an enema was administered. It acted freely without any injury to the newly-formed perineum. Scarcely any traces of the operation could be observed; the linear cicatrix, from the anus to the new commissure of the vulva, resembled the raphé. The four punctures of the pins appeared like small white dots on each side of the linear cicatrix.

On the 18th January, 1854, the patient walked about the room without the slightest inconvenience, and the next day met me in the parlour.

CHARLESTON, S. C., April, 1854.

ART. IX.—*Cases of Ovariotomy.* By ALEXANDER DUNLAP, M. D., of Ripley, Ohio.

CASE I.—Mrs. B., of Bracken County, Kentucky, aged 37 years, dark hair and eyes, bilious temperament, and the mother of five children, in September, 1852, came to consult me in reference to an enlargement of her abdomen, which had been supposed to be ascites. A careful examination, and the history of the case, showed it to be ovarian dropsy. I stated to her that medicine could not cure her, that tapping would only afford temporary relief, and that the extirpation of the tumour was her only chance for a cure; at the same time I told her the dangers of the operation, and that her chances of recovery would only be about three out of five, and left her then to decide that question for herself. She returned home. Her attending physician, still adhering to his former opinion—that she had ascites—tapped her. The enlargement of the abdomen was entirely removed by the operation, but in a month it had regained its former size, and she was again tapped. After this operation a tumour was discovered remaining in the abdomen. She was subsequently tapped twice. The amount of fluid drawn off by tapping I could not ascertain. In March, 1853, she sent for me to perform the operation for the removal of the tumour. I found her very much reduced in flesh; the abdomen enormously distended; fluctuation distinct in every part, with the exception of the right iliac region. She was most of the time confined to her bed, yet free, as I judged, from any organic disease, except that of the ovary. I determined to operate, which I did, after preparing her system, on the 24th of March, 1853, assisted by Dr. J. S. Bradford, of Augusta, Ky., in the presence of a number of medical gentlemen and students. The patient having been placed upon a suitable table, with her shoulders elevated, and her feet resting upon a chair at the end of the table, and brought under the influence of chloroform by Dr. Woodward, of Ripley, O., I commenced by an incision in the median line, through the integument and cellular tissue, from the umbilicus to a point near the symphysis pubis, by one stroke of the knife; then, by a careful dissection at one point of the incision, I opened the peritoneal sac, which I found free from adhesions at that point. By the use of my finger as a director, an opening was readily made into the peritoneum, corresponding with the one in the integument. The opening thus made was nearly twelve inches long. The adhesions, which were but slight, were then broken up by the hand (in which Dr. Bradford assisted me), excepting at the points where paracentesis had been performed, which I separated by the knife. The

tumour was then opened and emptied of its contents without allowing any of the fluid to escape into the cavity of the abdomen. Having raised the sac, together with a fleshy tumour about the size of a child's head at birth, from the cavity of the abdomen, it was supported by Dr. Bradford, while I proceeded to tie the pedicle by passing a needle, armed with a double silken ligature, through it, and tying one around each half. I then separated the pedicle, cut one end of each ligature close to the knot, and brought the other out and left it in the lower end of the incision. The cavity of the abdomen contained about a pint of fluid, and a transparent clot or coagulum floating loose without any attachment, which I suppose would weigh about eight ounces; this was removed, and the fluid carefully sponged from the abdomen. The lips of the wound were then brought together by five interrupted sutures, the intervening spaces supported by adhesive straps; lint was then applied, with a compress and bandage, and the patient placed in bed. The time occupied in the operation and dressing was twenty-eight minutes. On applying cold water to her face, she immediately aroused as from a sweet slumber, perfectly unconscious of what had been done. The pulse was 80, and had undergone no perceptible change since the commencement of the operation.

7 o'clock P. M.—Two hours after the operation, patient complained of pain in the lower part of the abdomen; pulse 88, and increasing in volume. Gave a pill 3 grs. of ext. hyos., and drew off twelve ounces of straw-coloured urine. 2¼ o'clock P. M.—Pulse 88; skin natural temperature; drew off six ounces of urine.

The following notes were taken by my office students, E. R. Bell, and J. P. McClanahan, who stayed with and nursed the patient throughout:—

3 o'clock P. M.—Patient complains of pain in the back and bowels. Repeated the pill of ext. hyos. 5 o'clock P. M.—Pulse 84; uneasy sensation at the stomach; pain in the bowels; feet cold. Hot applications to the feet, and repeated the ext. of hyos. 6 o'clock P. M.—Vomited a quantity of fluid of a dark green colour. 7 o'clock P. M.—Vomited again; pulse 100. 9 o'clock P. M.—Patient restless; complains of thirst; skin natural temperature; pulse 98, and compressible. Gave a pill ext. of hyos., and toast-water. Drew off seven ounces of straw-coloured urine.

March 25. 5 o'clock A. M.—Patient has rested well; pulse 84; temperature of skin natural. Drew off sixteen ounces of urine. 8 o'clock A. M.—Vomited a worm. 11 o'clock A. M.—Patient restless; has been vomiting; pulse 84; drew off five ounces of highly coloured urine; gave a teaspoonful of camph. tinc. opii. 4 o'clock P. M.—Has rested well; pulse 84; drew off four ounces of highly coloured urine.

26*th*. 2 o'clock A. M.—Rested well, until within the last two hours; complains of severe griping pains in the bowels; has been vomiting; gave a teaspoonful of camph. tinc. of opii; she vomited soon after, throwing up a large worm. 10 o'clock A. M.—Complains of distension of the bladder; pulse 84; drew off six ounces of urine. 2 o'clock P. M.—Found her in fine spirits; pulse 84, full and soft; tongue clean; appetite good; has no swelling or tenderness of the abdomen. Dressed the wound; found it uniting by the first intention; removed two of the sutures, and ordered her clothes to be changed. Diet—toast and rice-water. Ordered an enema to be given to-night to move her bowels. 8 o'clock P. M.—Resting well; drew off eight ounces of urine, and gave an enema. At 9 o'clock P. M. repeated enema, which produced a slight operation from the bowels. 10 o'clock, P. M.—Complains of pain in

the bowels; pulse 104, and full; skin warm and dry; some thirst. Gave a pill composed of blue mass, rhei, and ext. of hyos.

27*th.* 6 o'clock A. M.—Has rested moderately well, and has taken three of the blue mass, rhei, and extract of hyos. pills during the night; pulse 90; no evacuation from the bowels. At 10 o'clock A. M. gave two ounces of Rochelle salts. 2 o'clock P. M.—Repeated the salts; at 8 P. M. gave an enema. 10 o'clock P. M.—Had a free evacuation from the bowels; passed her urine without the use of the catheter; pulse 84.

28*th.* 6 o'clock A. M.—Rested well all night; pulse 84.

29*th.* 8 o'clock A. M.—Resting well; pulse 84; tongue clean; appetite good; has had a good night's rest. Dressed the wound; found it united by the first intention; removed the remaining sutures. It is discharging healthy pus from around the ligatures. During the night, the patient turned upon her side, producing no unpleasant feelings.

It is unnecessary to detail her symptoms further. From this time her recovery was rapid, and uninterrupted by any unpleasant symptoms. On the thirteenth day after the operation she walked across the room with assistance. I received the following note from Dr. Ferree, of Rock Spring, Ky., to whom I am greatly indebted for his kind attendance, with Bell and McClanahan, on my patient during my absence.

April 14, 1853.

I visited Mrs. B. this morning; found her in an improved condition, strong, and well enough to sit up half her time, and to walk into the adjoining room without assistance. I removed the ligatures this morning without the least difficulty or pain. I told her my attendance would no longer be necessary.

J. V. Ferree.

It was the left ovary that was removed. The weight of the tumour was thirty-seven pounds. Not more than four ounces of blood were lost in the operation.

Case II.—Mrs. F., of Clermont County, Ohio, aged 46 years; menstruation had ceased in her fortieth year. Had received an injury in her left side seventeen years ago, and had felt the effects of it ever since. About three years since, she discovered a small tumour, about the size of her fist, floating loose in the lower part of the abdomen, which had gradually enlarged. At the time she visited me, April, 1853, I found it filling the whole cavity of the abdomen, and greatly distending it. Fluctuation was distinct in every part. After examining the case, and hearing her history of it, I told her the disease was ovarian dropsy, and that her only chance for a cure was an operation for its removal. I stated to her the dangers she would have to undergo, and gave her Dr. Atlee's tables of ovariotomy, from which to make up her mind in reference to an operation. Her general health up to this time had been good, and although she was greatly reduced in flesh, I told her I could see nothing in her case that would deter me from operating, if she wished it. I gave her directions for medicine and diet, to prepare her system in case she determined on an operation. After returning home and consulting her friends, she sent me a note, stating that she wished the operation to be performed on the 17th of May, 1853. Accordingly, on that day, assisted by Dr. J. T. Bradford, of Augusta, Ky., and in the presence of a number of medical gentlemen and students, I performed the operation in the same manner as in the case of Mrs. B. There was but one slight adhesion to the omentum, which I divided by the knife. The patient did not come under

the influence of chloroform well, and had to be held during the operation, which lasted seventeen minutes, including the dressing; but she was entirely unconscious of pain. Pulse, on placing her in bed, 84, and had undergone no change during the operation. Complained of being sick at the stomach, which soon passed off, and which I supposed to be the effect of the chloroform. Six hours after the operation she complained of severe pain in her stomach, which readily yielded to ¼ gr. sulph. morph. Her urine had to be drawn off by the catheter for three days. Bowels were moved on the third day by medicine. Catamenia made their appearance the second day, and continued for three days. The sutures were removed, two on the third day and two on the fourth. The wound healed by the first intention; she was able to be on her feet the fourteenth day after the operation. No unpleasant symptoms occurred during the progress of the case. The ligatures came away on the twenty-seventh day after the operation. The tumour consisted of one large sac, to the inner surface of which were attached the small ones, the largest of which would probably contain one-third of a pint. The walls of the sac were thin, and of a very even thickness throughout. The sac and its contents weighed thirty-one pounds. It was the left ovary that was removed. The length of the incision made was ten inches; very little blood was lost during the operation. She has since enjoyed uninterrupted good health, and is now more fleshy than ever before in her life.

In the *Louisville Journal* of September, 1853, may be found a short account of an operation in which I assisted Dr. J. T. Bradford to perform, on the 17th day of June, 1853. Tumour weighed forty-one and a quarter pounds. Patient recovered without an unpleasant symptom. This makes the fifth case of ovariotomy in which I have been concerned, four on my own responsibility, and this last one in connection with Dr. J. T. Bradford. The last four have perfectly recovered.

Mrs. H., of Brown County, Ohio (whose case was reported in the *Western Lancet* of June, 1851), has since given birth to a healthy male child.

The one operated on in 1843 (this case was never reported, being refused publication by a western journal), died on the seventeenth day after the operation from diabetes. The wound had nearly healed by the first intention. No tenderness existed over the abdomen. No *post-mortem* examination was had in the case. The tumour was cystiform, and weighed forty-five pounds. At that time I was opposed to the operation, and only performed it at the urgent solicitation of the patient and her friends. The case, although unsuccessful, satisfied my mind that the operation was a justifiable one, and that, had it been performed earlier, would have terminated favourably. The foregoing cases were all cystiform. The pedicles were small. From the history of the cases, I judged no great amount of peritoneal inflammation had previously existed, giving me ground to believe that the adhesions to the surrounding parts were not extensive or firm. This I consider an important point in the diagnosis to justify an operation. During the past year, Dr. Bradford and I have been applied to for relief in seven other cases of diseased ovary and fibrous tumours of the uterus; in most of these the patients have been willing to submit to an operation, and some of them were even anxious and urgent to have it performed. But we have refused to operate for various reasons—some on account of obscurity of the diagnosis; others from the inflamed state of the peritoneum, and the fibrous character of the tumours.

In answer to the question, Is ovariotomy justifiable? I would unhesitatingly say yes, where the case is clearly made out to be cystiform in its character, the tumour free, or but slightly adhering to surrounding parts, and

the general health of the patient such as would justify any other major operation. The difficulty of diagnosing those diseases is, I consider, the great obstacle to recommending the operation, and not the opening of the peritoneum. But because we are not able to make a clear diagnosis in every case, are we to abandon the whole class of patients to become the prey of quackery? I believe it to be considered on all sides that medication has not, as yet, been able to reach those cases.

Two cases of spontaneous rupture of the sac have come under my notice; in one, the patient was under preparatory treatment for an operation. While in the act of stooping she felt something give way, and immediately experienced the sensation, as she described it, as if boiling water had been poured into her bowels. She died in two hours after the accident occurred. The *post-mortem* examination showed a rupture in a small sac, which would contain about a pint. The whole tumour, which was cystiform in character, weighed thirty-two pounds.

The other case was more fortunate. A tumour had been growing gradually in the lower part of the abdomen until it attained the size of a child's head one year of age. One night she was awakened by a burning sensation in her bowels; on placing her hand upon her abdomen she found that the tumour had disappeared. In the morning, as soon as she arose, she had occasion to evacuate her bowels, when she discharged about a gallon of straw-coloured fluid. In a short time, however, the tumour reappeared. The discharge, the second time, was not so rapid as at first, and at the same time that she had the watery evacuation from the bowels, she also had large discharges of water from the bladder, in which some traces of blood could be discovered. Once or twice in the course of two months she could feel a return of the tumour, but which disappeared upon a discharge of fluid from the bladder and bowels. Since that time, now nearly four years, there has been no return of the tumour; but she has scarcely passed a day without passing more or less wind from her bladder. Within the last six months I have frequently examined the evacuations from the bladder; I found them to possess the same appearance and odour as that passed from her bowels. This, however, is only when she is labouring under diarrhœa. Her life is one of great suffering, and opiates are her only relief.

ART. X.—*Autoplastic Operation for the Removal of the Deformity produced by a Burn.* By A. CLARKSON SMITH, M. D., of Columbia, Lancaster County, Pennsylvania.

IN the autumn of 1853, I was consulted by John Munroe, æt. 20, in reference to a deformity of the neck, produced by a burn he had received a year previously.

On examination, I found the "tissue of the cicatrix" unattached, for the most part, to the superficial fascia, being movable, and easily raised from the parts beneath. The tissue was thick, and composed of dense fibres crossing each other, and interlacing, covered by a delicate cuticle. Small cavities, formed by the crossing of the fibres, existed here and there over its surface, at the bottom of which slight adhesion was found with the fascia beneath.

The cicatrix extended from the inner edge of the mastoid muscle, of the right side, along the course of the lower maxilla to a point midway between the chin and angle of the left side of that bone, involving the integuments of the neck to a point half an inch below the thyroid cartilage, following a line drawn from this point to the acromial third of the clavicle. The parts seemed firm and rather pale, though somewhat sensitive. The appearance of vascularity was not such as to cause any apprehension of much hemorrhage. Altogether, the patient seemed in a condition in which an operation would be most likely to succeed; accordingly, I advised him to submit, stating, at the same time, the risks of a failure. After weighing the matter for a length of time, he concluded to have the operation performed, and, on the 2d of January, placed himself under my care for that purpose.

On the 4th inst., assisted by Dr. B. Rohrer, now of Germantown, Pa., and Dr. Mahon, of this place, I operated. The patient was placed on a table, with his head slightly inclined backwards. A mixture of chloroform and ether was exhibited until completely under its influence, and then, being firmly held by assistants, an incision was made, a line above the upper margin of the cicatrix, from the edge of the mastoid muscle of the right side to the termination of the cicatrix on the left. Getting under the "inodular" tissue, it was carefully dissected from the fascia, until the entire mass was completely removed. There was but little hemorrhage, and no vessel requiring the ligature was cut. A flap, sufficiently large to allow of considerable contraction, was taken from the breast (the head being still inclined backwards), and carefully applied by means of the interrupted suture and adhesive strips. The edges of the wound on the breast are approximated, as nearly as possible, by adhesive strips, and then covered with soft lint steeped in olive oil. The connection between the transplanted integument and its original location was preserved by a strip of integument an inch and a half in breadth. This was so loosely twisted as not to interfere with its circulation. The flap was covered with oiled silk, an opium pill given, and the patient left in charge of a nurse for the night.

January 5. Patient rested quietly during the night; complains of soreness in his extremities, produced, doubtless, by the struggles of yesterday, and a feeling of stiffness in his jaw. Pulse 88. Lymph thrown out abundantly around the edges of the wound. Ordered, pil. opii gr. j, ter in die, and milk diet.

6*th.* Passed a restless night; complains of a sense of fulness in the epigastrium; much pain in the wound on the breast; tongue coated; pulse 110; discharge of sanguineous fluid from the inferior portion of the flap. Removed some of the adhesive strips which had become loosened, and applied fresh ones; also redressed the breast, and in place of the oiled lint applied a slippery elm poultice. Ordered sennæ, mannæ, mag. sulph. āā ℥ij; semen. fœniculi ℥ss; aquæ fervent. Oss. M. ft. haust.

7*th.* Symptoms much improved; pulse 95; little or no pain in the wound on the breast; flap still adhering; cuticle vesicating. To prevent this, coated the entire flap with collodion.

10*th.* Removed the dressings from the neck and breast. Adhesion has taken place between the flap and surface beneath. The exclusion of the air by means of the collodion checked the vesication. A slight suppuration where the stitches were drawn through induced me to remove them. The parts were still supported by a few adhesive strips and a slight bandage passing around the neck and over the head.

From this time he rapidly recovered, and at the end of three weeks was able

to leave the house. So perfect has been the union of the parts that scarcely a vestige of the line of adhesion remains superiorly.

The connection between the flap and breast was severed at the expiration of three weeks, and the patient discharged.

Remarks.—Of the importance and utility of this class of operations there can be little doubt; and the profession owes much to the ingenuity of Prof. Mutter in establishing it in this country. By this means, unsightly disfigurations can be removed, and deformities, oftentimes so great as to interfere with the ordinary duties of life, remedied. Of course, there are many circumstances to be taken into consideration before making the attempt; and among these the most important are the *situation*, *extent*, and *depth* of the inodular tissue, and the condition of the patient. Reasoning on the established principle that perfect rest is necessary for the adhesive process, we should conclude that where this cannot be obtained we should not be justified in making the attempt; and yet, in the above case, the operation was entirely successful in a situation where perfect rest *could not* be obtained. The motion of the thyroid cartilage in swallowing, and constant throbbing of the carotid artery, increased by the irritation in its vicinity, precluded the possibility of obtaining this desideratum.

Where the cicatrix is extensive, it would probably be better to remove a portion at one time, inserting sound skin, and after adhesion has taken place, to finish it at a second operation. Where the fascia and muscles are involved, the operation becomes more formidable, and the chances of success are diminished; but, even in these cases, an attempt to relieve is oftentimes attended with the happiest results.

Columbia, Pa., *April*, 1854.

Art. XI.—*Cure of Laceration of the Urethra.* By Dr. J. Gautier, M. D., of Tuskegee, Alabama.

James Hall, aged 35 years; has light hair, blue eyes, fair complexion; a sound and vigorous constitution; weighs about one hundred and fifty pounds. In May, 1852, he was engaged as a deck-hand on board of a steamboat running from the city of Galveston to Brazoria, and while in the latter port, and engaged in discharging freight (at night), he attempted to cross over the hatch, and in doing so missed his footing, and fell astride of a square bar of iron extending across the hatch, upon which the doors rested. Laceration of the urethra, and great contusion of the perineum resulted. The injury occurred about 10 o'clock at night. I did not see Hall until the next morning, between 8 and 9 o'clock. He was then complaining of excruciating pain, with great distension of the bladder, and an inability to pass urine. By the forcible contraction of the bladder, occasionally a few drops of bloody urine would pass through the penis. On investigation, I at once concluded that there was laceration of the urethra. The only case of the kind I had ever

seen before, was in a little boy, treated in the Pennsylvania Hospital, under the care of Drs. Norris and Fox, in 1850. The experience furnished by the case, together with the valuable instruction given by the doctors above mentioned, rendered the treatment of this case, to my mind, positive and clear.

At once, I attempted to introduce a silver catheter into the bladder, but could only pass it up to the seat of laceration. After working with him an hour or two, and trying every size of gum-elastic catheter, I at last succeeded in introducing one of the latter, of very small size, into his bladder, and drew off by measurement twenty ounces of urine considerably coloured with blood. Having got a catheter beyond the point of laceration, I knew, from the facts of the case seen in the Pennsylvania Hospital, that it was wise to keep the instrument as long as possible in the urethra. On the second day, I had to remove the catheter; but I immediately introduced another of a larger size.

The perineum was kept constantly bathed in cold water, or a solution of sugar of lead. Hall had high fever and costive bowels, and saline purgatives were administered and an antiphlogistic treatment adopted. In a few days his fever subsided, and he was doing well. In six or eight days after his injury, I was able to introduce a large silver catheter into his bladder.

From the time of his injury, I have endeavoured to make it an established rule with Hall that he should never attempt to pass urine while the catheter was out of his bladder. On the morning of the fifteenth day of the injury, professional duty compelled me to be absent, and during my absence, Hall took the catheter from the urethra, and left it out for several hours. When he wanted to make water, he was unable to introduce the instrument. As he was suffering from distension of the bladder, another physician was sent for, and Hall was told to pass water *without* the catheter.

On my return home in the afternoon, I went immediately to see my patient. I found him in a condition vastly more dangerous than he had been upon the morning of his injury. The penis, scrotum, perineum, and cellular tissue about the pubis, were enormously distended from infiltration of urine. He had high fever, and his mind was considerably disturbed. Some of the bystanders supposed him to be dying. With considerable difficulty I again succeeded in introducing a catheter into the bladder. I then freely scarified the scrotum, to allow the infiltrated urine to escape. In a few days, mortification of the scrotum took place, and both testes were left entirely denuded of skin and cellular tissue, hanging only by the cords. The posterior and under part of the penis, and a portion of the perineum, were in a state of sphacelation. About two inches of the urethra was destroyed. From the destruction of the perineum, under part of the penis, and the urethra, at least two inches of the silver catheter could be seen. The bulb of the urethra was not injured, the destruction being anterior and posterior to the bulb. To add still another pang to Hall's misfortunes, one day, when asleep, flies deposited their ova on his testes and perineum. In the course of four or five days, at least fifty large maggots were taken from the perineum. My patient was now troubled with constant hectic fever, and greatly emaciated. In this condition, I had but little hope of his recovery. For ten or twelve days he lay feeble and prostrated almost unto death. I watched his case closely, removed all disturbing causes, and kept the catheter constantly in his bladder, removing it only to be washed. I gave him but little medicine, principally tonics. Rich, nutritious diet was allowed. The case was left as much in the hands of nature as possible.

About the latter part of June, Hall began to show evidences of recovery. His general health was gradually improving. Nature was throwing off the

putrid mass, and beginning to reproduce new tissue. Indeed, the rapidity with which the parts were restored to their normal condition was truly astonishing. In less than six weeks from the commencement of the formation of new tissue, the testes were enveloped in a new scrotum; the penis, urethra, and perineum were entirely restored.

By the latter part of August, Hall was well, and free from deformity. He could retain his water as well as he ever did, and void it *per vias naturales*, and with ease. He has not used a catheter since the 12th of August. When I saw Hall again, early in September, he was riding an unbroken mustang horse; and he assured me he was perfectly well, and had as much strength, and as perfect use of himself, as he had before the fall.

Tuskegee, Ala., *July* 12, 1854.

[*Note by the Editor.*—In connection with the above interesting case, we will refer to some observations which we published in the number of this Journal for Feb. 1837, on accidents of this character, their nature and mode of treatment, illustrated by a large number of cases.]

Art. XII.—*Case of Epilepsy, treated by Ligation of the Common Carotid Artery.* By Joseph B. Brown, Assistant Surgeon U. S. Army.

"*June* 2, 1848.—Miss Elizabeth W., æt. 22, has been subject to attacks of epilepsy since her 17th year. Some four years before this time, she first felt a strange sensation in her right forearm; it gradually passed above the elbow, and now appears at the intersection of the omohyoideus by the trapezius, and precedes the convulsions as a true *aura*, being described by the patient as a stream of warm fluid issuing from this spot. Menstruation was normally established before the disease became confirmed. The patient is now constantly afflicted with convulsions, which vary in character and intensity; those occurring in the night being of the most violent convulsive character; while those during the day are generally less aggravated. Sometimes a week or two intervenes without any attack; but she states that she has had as many as twenty-four of the fits in the twenty-four hours. Her face bears constant marks of the violent injuries she receives from falls against articles of furniture, when prostrated, as she often is, without warning, while engaged at her usual avocations, or walking across the floor. She is remarkably intelligent, and of excellent physical development.

"Her attending physician, Dr. Z. Pitcher, of Detroit, despairing of affording her any relief from the ordinary means of medical treatment, which have been properly and perseveringly tried, has consulted with me as to the propriety of attempting a cure by a surgical operation. I saw the patient to-day, and finding no sign of disease external to the brain, to which the convulsions could be referred, agreed with Dr. P. as to the propriety of attempting to relieve her by applying a ligature to the common carotid artery of one side. *It was decided not to use chloroform*, as generally hazardous in operations of importance about the head, and for such a one as this, and in such a case, as particularly imprudent.

"*5th.* Operated to-day, assisted by my brother and Dr. Alfred Brush, Dr. Pitcher and some other medical gentlemen being present. The vessel upon the right side was selected, and tied *above the omohyoideus*, a slight enlargement of the thyroid gland interfering with the artery below. *No chloroform was used.* Pulsation immediately ceased in the temporal and facial arteries of the side, upon tightening the ligature, but no perceptible change in the feelings of the patient was experienced."

The above is an extract from my note-book, kept at the time. The relief afforded by the operation was immediate. The ligature came away on the twentieth day, long before which time pulsation was re-established in the right temporal. Constant service upon a remote frontier has until recently prevented me from hearing the subsequent history of the case. Dr. Pitcher reports (July, 1853) that "the aura has never returned, neither have the grave and convulsive forms of the disease; but within the past year, when exercise is omitted, and any unpleasant mental disturbance takes place at the same time, very slight returns of the *petit mal* occur, but never to occasion loss of consciousness. The health of the patient is now perfect. Her temper, which had been rendered irritable by the disease of her nervous centre, is very much improved, and her appreciation of existence greatly enhanced."

For more than *three years* she was exempt from seizure of any kind.

The history and statistics of ligation of the carotid have been made the subject of a valuable essay by Dr. Norris, of Philadelphia.[1] His tables show that this operation, though several times performed for cases ef epilepsy, has never been successful for the cure, but all have recovered from the effects of the operation. He says that "the idea of curing epilepsy by tying the carotid seems to have been founded on false principles." It is certainly difficult to reason upon this matter, when the pathology of epilepsy, the species designated as *centric epilepsy* especially, is so imperfectly understood. Should it depend upon a functional derangement of the relation between the arterial and venous circulation within the head (as probably in the above case), a measure which would so materially diminish the force and volume of the blood sent to the brain as ligation of the carotid, would seem, if not expressly indicated, at least to demand a trial. It is true, the difficulty of diagnosing cases in which this derangement operates must ever exist; but the above case certainly shows that the most aggravated forms of the disease may arise from some such cause, and that mechanically checking the volume and force of the arterial blood poured into the brain may restore the balance and effect a cure. The operation has never proved fatal, though uniformly, till the present case, unsuccessful as a cure; but with the experience of this *one*, productive of so much benefit, restoring the patient from a state of existence too horrible to endure, to the comfort of health, it seems to me that, in proper cases, we should not be justified to abandon it entirely.

Note.—This case has been recently reported by Dr. P. in the *Peninsular Medical Journal*, but the report is evidently drawn up from recollection, as it contains several inaccuracies.

Ringgold Barracks, Texas, Jan. 28, 1854.

[1] Am. Journ. Med. Sciences, July, 1847.

FISKE FUND PRIZE ESSAY.

ART. XIII.—*"Neuralgia; its History, Nature, and Treatment."* By CHARLES W. PARSONS, M. D., of Providence, R. I.[1]

THE writer cannot suppose that a detailed description of the symptoms or varieties of locality of neuralgia is expected in this Essay. It is believed that the objects for which this generous foundation was established, and is now administered, will be best consulted by brevity, and a strictly practical aim in the choice of topics. We shall, therefore, confine ourselves to the following subjects:—

I. The General Topography of Neuralgia.
II. Its Causes.
III. Its "Nature," or Pathology.
IV. Its Treatment.

I. By *general topography* we mean an account of the kinds of nerves, or divisions of the nervous system in which neuralgia exists; and also some general statements as to the situations in the course of any nerves, which are its ordinary seats and centres of radiation. We do *not* mean to include a list of all the individual nerves that are sometimes attacked.

Neuralgia is best known as occurring in the cerebro-spinal nerves. Does it ever attack those of the ganglionic or sympathetic system? Experiment has proved that mechanical irritation of these nerves does not elicit signs of pain till after a time, when congestion or vascular excitement is produced, and then acute pain is caused by such irritation.[2] We might expect then that other irritating causes, such as poisons circulating in the blood, or some derangement of different functions, might produce an analogous sensitiveness in the nerves of the sympathetic system. And such is the fact. The analogy of many painful visceral affections to external neuralgia is as complete as the nature of the case admits; though the sensations cannot be described so definitely, because the nervous fibres have not so direct and continuous a relation

[1] The Trustees of the Fiske Fund, at the annual meeting of the Rhode Island Medical Society, held at Providence, June 7, 1854, announced that the premium of fifty dollars offered by them in 1853, for the best dissertation on *Neuralgia, its history and best mode of treatment*, had been awarded to the author of the dissertation bearing the motto,

"What is writ is writ,
Would it were worthier!"

And upon breaking the seal of the accompanying packet, they learned that the successful competitor was Charles W. Parsons, M. D., of Providence, R. I.

JOSEPH MAURAN, M. D.,
SYLVANUS CLAPP, M. D., } *Trustees.*
ARIEL BALLOU, M. D.,

S. AUG. ARNOLD, *Secretary.*

[2] Brachet, Recherches Expérimentales sur les fonctions du Système Nerveux Ganglionaire. Paris, 1837.

with the sensorium as the cerebro-spinal nerves have. These affections, gastralgia, nephralgia, &c., are not necessarily or usually connected with any change of structure. In angina pectoris, one of this class, ossification of the coronary arteries, fatty degeneration of the heart, and other organic alterations have been found, but by no means uniformly; they are not, so far as we now know, essential parts of the disease. The attacks of these "visceral neuralgiæ" are generally marked by little constitutional disturbance, in proportion to their severity. They are paroxysmal, and often the fits arise without any obvious exciting cause. Though very obstinate, they sometimes subside suddenly, we know not why, and their long continuance does not always seriously affect the general health, except by the mere amount of suffering. They occasionally alternate with external neuralgia. If the attacks of those which occupy glandular organs sometimes end in a sudden flow of secretion, the same is common also in the analogous affection of the fifth pair.[1] It is true that there is generally some functional derangement accompanying the pain, and, of course, the distinct character of these internal neuralgiæ, as pure *nerve-pains*, is not so perfectly marked, nor so uncomplicated, as in pains of the superficial nerves.

How far the acquired sensitiveness of ganglionic nerves depends on their connections with the cerebro-spinal system, is a question properly of physiology, which we will waive.

Among cerebro-spinal nerves, the most superficial ones are said to be the most common seats of well-marked neuralgia. But to this there are many exceptions. Nerves which plunge into the thickness of dense organs have their branches closely interwoven with the tissues around, so that the sensorium can only distinguish a diffused pain. Such affections as neuralgia of bone and of the testis (when the spermatic cord is not involved), are called neuralgic, because pain is altogether out of proportion to the slight and inconstant anatomical changes. But in the superficial nerves, pain can be tracked along the nervous trunks, so that a patient can study their anatomy on his own person. Facial, dorso-intercostal, and sciatic neuralgia are believed to be the most common forms.

But certain points appear to be the peculiar and favourite rallying points, or foci, of this disease. This part of its history has been particularly investigated by Valleix,[2] and his observations are among the most interesting additions recently made to our knowledge of this affection. The "focal points" of external neuralgia are generally in the following situations:—

1. The place of emergence of a nervous trunk, as at the supra-orbital and infra-orbital foramina, the crural arch and ischiatic notch, &c.

2. The points where a nervous branch comes out through the muscles, to ramify in the integuments. For example, where the posterior branch of the second cervical appears behind the mastoid process, or where those of the lower spinal nerves become superficial.

3. The points where the terminal branches of a nerve expand in the integuments, as at the anterior extremity of the intercostal nerves.

4. The points where the nervous trunks become superficial during their course, as where the ulnar nerve passes the inner condyle.

These points are peculiarly liable to suffer in neuralgia; they are, almost

[1] The secretions may be apparently *perverted* during the fits. A patient of Macculloch suffered from neuralgia in different superficial nerves, and at last from nephralgia, accompanied by diabetes mellitus. This "was strictly paroxysmal, or the morbid secretion of sugar commenced with the fit, and entirely disappeared in the interval."

[2] Traité des Névralgies, &c. Paris, 1841.

without exception, sensitive on pressure in the intervals between the attacks, but much more so during the attacks; at these places there is, in nearly all cases, a constant dull pain, or bruised, tense, or numb feeling; and from these points the lancinating pains diffuse themselves, either by spreading along the nerve in a continuous line, or leaping over to other spots. Pressure sometimes merely increases the dull pain in these points, sometimes causes twinges along the nerves. The extent of surface over which this tenderness exists is very small, often less than a square inch. The tenderness may be absent at one examination, and be found on a subsequent one; it would appear sometimes to intermit. These facts explain, wholly or in part, the various opinions of observers as to the effects of pressure. I have many times examined the points above mentioned as focal points, in facial and sciatic neuralgia, and have only once failed to ascertain that the patients had painful sensations there in the intervals of acute paroxysms, and that pain was produced by pressing these spots. Valleix mentions but one exceptional case—a sciatica, which had existed only twenty-four or thirty-six hours, and was very slight.

Neuralgia commonly occurs in the nerves which, in health, are conductors of sensation. Is it confined to these, or do some motor nerves ever become morbidly sensitive in this affection? The inquiry is interesting, because many cases of tic douloureux occupy the side of the face, and follow nearly or exactly the course of the branches of the facial nerve or portio-dura, a motor nerve. They are often believed to be seated in this nerve. Some are accompanied by spasmodic motions of the cheek, which gave rise to the name *tic*, and these are thought to show that the motor functions of this nerve are implicated. To suppose that the same nerve-fibres are at once sending an impression of pain toward the brain, and also a motor impulse from the brain to the muscles, would be in contradiction to all received notions of physiology. In one case—and perhaps many such have occurred—a competent surgeon considered a neuralgic affection to be seated in this nerve, the pains spreading over the cheek from the stylo-mastoid foramen, where this nerve emerges from the cranium. He divided this nerve near its place of exit, with the effect of paralyzing the muscles of that side of the face, with no mitigation of the pain. As to the inference drawn from muscular spasm accompanying the pain, it has been shown by contemporary observers, that real involuntary spasmodic movements are rare in tic douloureux.[1] After all, their existence is explained by the anatomical facts, which will be found, we believe, to account for all these phenomena.

Communicating fibres from the inferior maxillary nerve[2] join the facial near its point of emergence, behind the jaw, so that the latter contains threads whose origin and endowments are the same as those of the trifacial or fifth nerve. In some dissections, this branch from the fifth nerve has been found as large as the whole portia dura of the seventh. It has been shown by experiment that, when the fifth nerve is divided within the cranium, the seventh loses all traces of sensibility.

Nor is this all. The posterior branch of the second cervical nerve sends a division to the back part of the scalp, passing almost exactly over that point already named, where the facial nerve issues from the cranium. In many cases, the seventh pair was believed to be affected because the pains started from near the mastoid process. But in many of them it will be found that

[1] Rowland and Valleix. They occurred in four out of fourteen cases observed by Valleix, and in sixteen out of fifty-five, whose history he collected.

[2] From the ascending branch of the anterior auricular.

the starting-point is intermediate between the mastoid process and spine, exactly where this cervical branch comes out from the muscles to become superficial. Indeed, the same thing is mentioned by authors who did not interpret it as I would do. Halliday, in his Treatise before me, gives a case of "neuralgia of the facial nerve;" it is stated that the pains "set out from the occiput, a little above the nucha, between that and the mastoid process." There had been a softish swelling in the same spot. It is believed these anatomical circumstances may explain most, if not all the alleged instances of neuralgia of the facial nerve.

II. A consideration of the causes of disease is, in a practical point of view, one of the most important parts of its history. It is too common to pay exclusive attention to the structural changes, the visible effects of morbific agents, and to classify disease on such principles alone—an error which cannot fail to have its influence on treatment.

Of the local causes of neuralgia, the most frequent undoubtedly is exposure to cold and wet. These attacks, in my own observation, are most frequent in the earlier months of spring. A residence in cold, damp situations and climates is a predisposing cause. Bellingeri analyzed the history of forty cases, and found thirty-four of them attributable, as he believed, to exposure of this kind. A cold, dry, and piercing wind, or sitting in a current of air, sitting long in wet clothing, are the exciting causes most often and distinctly recognized. Indeed, any agent which deranges the circulation of the surface for a long time, so that a proper reaction is not established, is capable of inducing attacks.

The forms of neuralgia most often produced in this way are, I believe, facial and sciatic. The branches of the trifacial or fifth nerve are distributed over a large extent, many of them are quite near the surface, and the part they supply, the face, is habitually most exposed to changes of temperature. The sciatic nerve is deeply covered, but its fibrous envelop is of denser structure than the neurilemma of other nerves, more allied to the textures usually invaded by rheumatism; and we find, accordingly, that it is very liable to an affection completely analogous to rheumatism in its nature and causes. Sciatica prevails in cold, wet seasons, in stormy and changeable weather, and in low and damp places; also, according to European observers, in sailors and fishermen. The severest and most intractable cases I have seen were in Irish labourers, who had been employed in wet and underground work, and wore their clothing wet for hours.

Exposure to heat has sometimes produced neuralgic attacks, as in the face of a cook who had to stand over a hot fire a great deal. In connection with this may be mentioned a case I have lately seen in a man whose face is often exposed to the vapours of hydrochloric acid. He says that many of those working in the same business have eruptions on the face, which he has not. His neuralgia has lasted six years, and occupies the terminal branches of the superior maxillary nerve.

Direct mechanical injuries are not among the most common causes. Bellingeri analyzed forty cases, and found but two of them from injury. Halliday relates twenty-seven, and only two of these came from injury. In several cases proceeding from this cause, the disease has continued for months or even years after the accident; and the origin of the symptoms has been shown by their immediate subsidence upon making an incision over the part. One case in a girl of fifteen, began after a severe blow on the right temple, continued rather mild for the first two months, then ceased for two or three months, and came on again in severe neuralgic fits. No immediate cause for this recur-

rence could be ascertained, except that the catamenia had failed to appear. Relief was twice obtained by covering the cicatrix with lunar caustic. This point was so tender at times, that the slightest touch produced acute suffering, which at one time was followed by general convulsions. The catamenia were regularly established about thirteen months after the original injury, and the neuralgia rapidly improved after that time.

Venesection is known to have often produced neuralgia, both in the arm and leg. A puncture of the superior maxillary nerve at the infra-orbitary foramen, made for the purpose of applying galvanism, has caused it. In some cases of neuralgia from puncture, it has been proved that the nerve was pierced or partially divided, and a complete division at the wounded part, or about it, has sometimes given relief. But it is not certain that a nerve is touched in all these cases. The analogy of other causes, such as cutaneous eruptions, leads me to doubt whether any inflammation or great disturbance in the circulation around a number of the most minute terminal fibres may not serve as an excitant, and whether, therefore, we need always suppose the wound to have reached a nervous trunk. Dr. Rowland mentions a person who had neuralgia from leech-bites, applied to a bubo.

Neuralgia seldom begins immediately after the wound, or rather, as soon as the pain caused at the time has subsided. Such a fact is not unknown. One man had a severe laceration on the thigh, leaving some filaments of the sciatic nerve hanging loose. Shooting pains, with spasmodic twitchings, came on in about four hours, and were followed by numbness, &c. All these symptoms were passed in nine days. The disease rarely presented so acute a form. In general, neuralgia following external injuries, begins after cicatrization, or even some months later. With a view to prognosis and treatment, it is very important to distinguish between those cases in which the symptoms come on within a few hours after the injury, and those in which they manifest themselves after the wound has healed. In the former, we have more reason to think a wounded nerve has occasioned the pains, and we may suspect a partial division of the nerve. An operation is much more likely to give relief in these cases. "It is always to be feared," says Mayo, "when the nervous symptoms have been slow in coming (and, therefore, have been present for many days), that they are dependent not more upon the injured point of the nerve than on an irritation involving its whole length." Some of those which immediately follow a wound may be presumed to be inflammatory; as in the above-mentioned case which followed laceration.

It is important to bear in mind that an injury which had not been suspected of having involved any particular nerve, and perhaps had occurred so long before as to have been forgotten, may still cause acute nervous pains. For example, Dr. Rowland mentions a girl, aged sixteen, who had paroxysms of darting pain in the *left* temple and side of the head; and upon inquiry it was found that several years previously she had received a severe cut over the *right* parietal bone, which was long in healing, and this spot had been tender ever since. A large uneven cicatrix was discovered, when the hair had been removed, and a blister over this part relieved the pain for several weeks. The pains may be much more distant from the seat of injury, and even tenderness of the cicatrix may be wanting as a guide to their true source.

Pressure upon nerves is a more common local cause. It may be made by some foreign body, a tumour, an enlarged or displaced organ, a curved spine, stercoral concretions, the passage of the fœtal head, the pressure of crutches against the armpit, &c. In a neuralgia of the spermatic cord, pain was excited by flatus passing through the cæcum. Dr. W. E. Coale, of Boston,

witnessed a case of neuralgia, relieved on the appearance of a deciduous tooth, which had remained in the upper jaw till the person reached mature age.

Pressure by *bone* is a well-recognized cause. Sir Henry Halford may be thought to generalize from his observations a little too broadly; but as he gives facts for his authority, we will not make this topic a bone of contention. He thinks that "the disease (tic douloureux) is connected with some preternatural growth of bone, or a deposition of bone in a part of the animal economy, where it is not usually found in a sound and healthy condition, or with a diseased bone." He gives four cases, all of them interesting, and three of them pertinent here. A lady, aged forty, suffered severely from tic douloureux; it was observed that the attacks were frequently preceded by uneasiness in one tooth, which exhibited, however, no signs of unsoundness. The tooth being extracted, a large exostosis was observed at its root; and the lady never suffered more than slight attacks afterward, and those very seldom. In another, recovery took place after the completion of an exfoliation from the antrum maxillare. In a third, there was exfoliation of the alveolar processes, and relief occurred only after an apoplectic seizure. This may have depended on disease within the cranium, as in the case of Dr. Pemberton, related in the same connection by Halford, but which we may do best to consider under another head.

Tumours may form in the nerves, and become the radiating centres of neuralgic pain. Though not among the most common causes of neuralgia, still, tumours of nerves form an important part of our subject, and we will give a condensed sketch of the leading facts respecting them.

The only known cause of them is a wound or similar irritation of the nerve, at the point where they appear. Hence, they are divided into two varieties, the traumatic and idiopathic neuroma. They are generally ovoidal, their long axis parallel to the nerve, movable, not adherent to the skin. The majority of cases where but a single tumour occurs, are believed to be of traumatic origin; but under some unknown predisposing influence, many tumours, even hundreds, may exist in one person. The symptoms of neuroma, when seated in superficial nerves, correspond very well with those of neuralgia—constant sensations of tingling, &c., with exacerbations of acute, shooting pain, excited by atmospheric changes, disordered digestion, &c., as well as external pressure. The pain is by no means proportioned to the size of the tumour. It is more moderate at first, perhaps no more than a tenderness, with many ill-defined sensations along the course of the nerve. Traumatic neuroma is, of course, most common on nerves near the surface. In cases where a great many exist in the same person, they have been found in the nerves both of the sympathetic and cerebro-spinal systems.

The structure of these tumours is pretty uniform. They are probably never cancerous, and never act like malignant disease. Those coming from injury are generally solid, but either variety may be encysted. The solid tumour begins in the neurilemma, or cellular sheath of the nerve, or else in one of the finer sheaths enveloping particular fibres of the nerve; the nervous threads may generally be traced through it, by careful management; those which are not involved in it are generally pushed one side, or some of them are spread and flattened out on all sides, according to the exact situation where the growth commences. The neurilemma by its compression gives the ovoid shape. The section of a solid neuroma shows it to be fibro-cellular and homogeneous; it cuts smooth. Those in the shape of a cyst contain some kind of liquid. Rokitansky believes they serve as bursæ mucosæ.

A bulbous enlargement of this shape and structure forms on the nerves

after amputation, usually without occasioning pain. But some of the worst known instances of neuralgia have followed amputation, so that patients have more than once submitted to reamputation, for the chance of relief. "The adhesion of the end of the nerve to the cicatrix of the stump; its irritation by osseous spiculæ springing from the extremity of the sawn bone; the including of a nervous filament in a ligature; the occurrence of a conical stump, such are a few among the many circumstances which have been stated as the occasional causes of these terribly painful tumours."[1]

Carious teeth are believed to be a very frequent cause of tic douloureux. From my own observation, I should not expect much good from extracting teeth, unless the pains occupied the very nerve whose branches pass to a diseased tooth. Brodie says, in his lectures in the *London Medical Gazette*, that he never knew a case where genuine tic douloureux was relieved by the extraction of a carious tooth; and a very experienced dentist informed him that he had arrived at the same result. Still, it cannot be denied that neuralgia, even at a distance from the face, is sometimes caused by carious teeth. Bell met with a case "in a gentleman, who had for some time suffered from a neuralgic affection of the right arm; the paroxysms were subsequently observed to commence with pain in the second molar tooth, and to be excited whenever it was pressed or otherwise irritated. Upon the removal of the tooth, the pain disappeared." A pain running up the left arm to the shoulder, chest, and cardiac region, has been immediately cured by extracting a carious molar.

Neuralgia has sometimes been produced by stretching of nerves, without any laceration or wound, as where a heavy weight has been suddenly brought into such a position as to be supported by one arm. Sir Everard Home reported in the *Philosophical Transactions*, for 1801, the case of a gentleman, who received a violent sprain of the thumb by the weight of his body being thrown upon it, in saving himself when he was nearly thrown off of his horse. The attacks of pain extended up the arm; and the case proved very obstinate. A nerve was divided without obtaining a cure.

Various irritations and eruptions of the skin are sometimes curiously associated with pains in the neighbouring nerves. The congestion, or peculiar state of circulation in the minute vessels, which immediately precedes the appearance of eruptions, is more commonly marked by itching and diffused burning heat, but sometimes assumes a properly neuralgic character, particularly in herpes zoster, also sometimes in eczema and impetigo. These pains may last through the eruptive period, aggravated by everything that heats the surface. The most obstinate pains, I believe, that are connected with herpes, are those which follow its disappearance, and which are noticed by Dr. Bright in his lectures as particularly refractory to treatment. A recent writer, Marriott, mentions that he has observed many cases, which all subsided spontaneously after some weeks or months.

Carbuncle may cause neuralgia, in near or distant nerves, as shown by a case which was long under my own care. The carbuncle was a large and bad one, seated over the left scapula. Its appearance was preceded by severe pleurodynia on the same side; at the height of the suppuration there were paroxysms of pain in the subscapular nerve; and at that period the following symptoms first appeared. Pain and numbness were felt down the left ulnar nerve, particularly at the point where it passes beneath the inner condyle, and from the inner side of the wrist down the little finger, and half the next. The pleuro-

[1] This quotation, as well as most of the account of neuroma, is from an excellent monograph, "On the Pathology, Diagnosis, and Treatment of Neuroma." By Robert W. Smith, M. D., &c. Dublin, 1849.

dynia was very distressing, was always relieved by soothing applications to the carbuncle, and never so severe after that was freely opened. It disappeared long before healing was effected, but the pain in the arm continued. The spot was quite slow in healing, being exposed to continual fretting by the motions of the scapula; and while it continued open, the pains down the arm were constantly increased by anything that irritated the little ulcer, and relieved by soothing applications. After complete cicatrization, for a month or two if not longer, pressure on the scar would cause painful and strange thrilling sensations down the ulnar nerve. A second, comparatively small boil, on the corresponding point on the right side, caused much slighter and very transient suffering of the same kind, at the same points of the right ulnar nerve.

Inflammation of mucous membranes has been known to excite neuralgia in those nerves which supply the inflamed membranes. This has been observed in influenza particularly. I have just had a case under treatment, where the fits of pain shooting over the whole scalp and temple, recur about three o'clock every morning, with a good deal of regularity, and last till late in the forenoon, when they seem to be relieved by a copious discharge from the pituitary membrane. The pains were confined to the region of the frontal sinus—a severe headache, till the influenza had lasted about five days. There are some curious notices in authors of similar pains, caused by larvæ of insects lodged in the frontal sinus.

Neuralgia may have its origin in the nervous centres. It would be obviously out of place to give an account of all the affections of the brain or spinal cord in which it may arise as a symptom. Softening of the brain is more often indicated in its earlier stages by spasmodic contractions of the muscles, particularly rigid spasm, and by prickings or numbness; sometimes these are preceded or accompanied by shooting pains in the limbs. I do not know that these pains ever follow exactly the track of a nerve. Like symptoms are apt to be among the earlier indications of softening of the spinal cord.

Neuralgia is thought to be sometimes a symptom of cerebral congestion. Interesting cases are related by Dr. Copland, in his *Dictionary*. One, in a gentleman aged about fifty, was treated by him as depending on an active determination of blood to the head, and disappeared. In about two years it returned, and was followed by apoplexy and death. A lady about the same age was relieved by cupping on the back of the neck, but the pains soon returned, when carbonate of iron was prescribed by another attendant; after this "she immediately became maniacally delirious, afterwards hemiplegic, and she soon afterwards died." An autopsy showed intense congestion and signs of previous inflammation.

Similar statements will be found in the writings of Dr. Armstrong. He regarded congestion, or disturbed action in the vessels of the brain, as the most common cause of *tic douloureux* and of periodical headache.

In the *London Journal of Medicine*, August, 1850, is a case of pains which were unaccompanied by any signs of inflammation, and considered and treated as neuralgic; situated in a limited spot near upper and inner margin of right ilium. An unnatural fretfulness was the only proper cerebral symptom. The liver was enlarged, so as to be felt below the ribs, and this was thought to explain the neuralgia. These symptoms began in the autumn, the pain suddenly subsided about the middle of the following April, and two weeks after an attack of apoplexy occurred, which was fatal in five days. Autopsy revealed extensive softening of left side of cerebrum, with effusion of blood. The cessation of pain was probably owing to the progress of the disease in the brain.

Sir Anthony Carlisle mentions that a lady was relieved of infra-orbital neuralgia, by three bleedings within a week of one another. First, sixteen ounces were taken; three days later, twelve ounces; three days later, sixteen ounces again. She was entirely free from pain, and never had any return of it. She died two years after this, of apoplexy. These facts should not be forgotten in estimating the gravity of this disease, and in determining whether to give tonics.

In the case of Dr. Pemberton, there was disease of the bones within the cranium—an unusual thickness of the os frontis above its sinuses, and near its junction with the parietal bones; and in the falx, near the crista galli, there was "a small osseous substance about three-eighths of an inch in length, rather less in breadth, and about a line in thickness." After amputation, performed for neuralgia in the leg, the posterior surface of the spinal cavity has been found studded with cartilaginous and bony deposits.

It would be interesting to obtain a series of facts, to show how frequently neuralgic pains are connected with a tendency to serious disease in the nervous centres, and how far they may be premonitions of such disease. In persons approaching that age when apoplectic and paralytic affections may be feared, obstinate sciatica, not traceable to any obvious external cause, should always call our attention to the state of the brain. Facial neuralgia, too, has sometimes passed into severe periodical ache in the inside of the cranium, followed by cerebral symptoms. Before leaving this subject, we will quote the suggestive remarks of Copland, from whom we have already given cases in point. After referring to the external physical causes, cold and wet, &c. he says: "When neuralgia cannot be imputed to any of these causes, when it is occasioned by less manifest causes, when there is reason to suspect that organic lesion exists within either the cranium or spine—protracted disease may be expected, and the supervention of another malady, generally resulting from the progressive increase of the primary lesion, and of still more fatal tendency, may be anticipated although at a more or less remote period. In a very large majority of these cases, neuralgia terminates in some related malady, in a convulsive, epileptic, apoplectic, or paralytic seizure. From either of these the patient may recover partially, rarely completely, and be again attacked, but he seldom experiences the neuralgic affection, or at least in the same form or degree of severity. Of the several maladies into which neuralgia passes, palsy, generally in the form of hemiplegia, sometimes in that of paraplegia, when the lower extremities have been the seat of the affection, has been that most frequently brought under my own observation. Next to this, apoplectic, or apoplectic joined with convulsive seizures, have been noticed." In cases I have witnessed, where neuralgia was followed by palsy, I have not seen any relief ensue, except so far as the sensorium was blunted for the time against the perception of pain.

The following remarks of Simon, one of the ablest recent writers on pathology, are too interesting to be overlooked. He gives no specific facts to confirm them: "It is in these diseases of subordinate centres—chiefly of tertiary centres—that *neuralgia* so commonly arises; for by this term, when it is properly used, we denote the subjective experience of pain in a part where there is no equivalent objective disease; and in most of such cases, the disease is not in the nerve-tubules, but amid the gray nerve matter of the secondary or tertiary centre"—*i. e.* the cerebellum, optic thalamus, corpus striatum (secondary), or gray matter of the spinal cord, or sensitive ganglia (tertiary).

As this essay is intended to have a practical character, we will not go deeply into the vexed question of spinal irritation as a cause of neuralgia.

Certain cases of this disease in the face, and more particularly in the intercostal nerves and abdomen, are connected with tenderness alongside the spine—often confined to the surface over those intervertebral foramina which transmit nerves to the parts affected by pain. This tenderness is exhibited on pressure, or on applying heat or cold, as for instance by a hot sponge or the cold hand passed down the back; and also after some motions of the limbs or body. Pressure here will sometimes excite the painful or other peculiar sensations in distant parts. There is often a little œdema about the tender spots; and it is said the skin is at times more easily irritated by such applications as tartarized antimony than the surrounding parts. Lastly, many such complaints are relieved, or perhaps permanently cured, by leeching or counter-irritation over the neighbourhood where tenderness exists.

These facts have been ascribed to a state called "spinal irritation," of which only a very vague idea can be formed. It has been considered a very moderate sort of inflammation—proved to exist by the tenderness on pressure, and by the very prompt good effects of leeching, blistering, &c. As to this theory, it is enough to say that no pressure can, as a glance at the anatomy of the parts will show, be transmitted from our fingers on the outside to the spinal marrow itself. And the effects of treatment are by no means so constant as to make a fair ground for pathological inferences. They show, we think, that there is a state—temporary congestion it may be—of these parts of the spinal cord, which causes or keeps up the neuralgic, and other distant affections. But the relief is both very uncertain, and sometimes so surprisingly prompt as to show that some other condition than inflammation has existed.

The tenderness itself has often a neuralgic character. It is in many cases excited by a slight brush or change of temperature more than by firm pressure. Pinch up a fold of the skin over the intervertebral foramen, and in many of these cases the local sensitiveness will be evinced, and, what is more, the distant neuralgia will be waked up. This result is not constant, it is true, but neither are any of the phenomena of spinal irritation. We have seen (p. 418) that tenderness on pressure usually occurs, in neuralgia, at those points where the nerve issues, either from bony foramina, or through the mass of muscles, to come nearer the surface. All the facts of tenderness, &c. prove, then, is, that the posterior branches of the spinal nerves in question are affected together with the anterior, and that very probably the ganglia contained in the cord itself are the media of communication between them. It should be added, to illustrate the inconstancy of all these facts, that many of these patients have numerous tender points in different regions, a general sensitiveness—that the same spinal tenderness may be found without any neuralgia, or any of the other complaints which modern theorists have traced to the same source—and, conversely, that just such symptoms, apparently owing to some peculiar state—call it irritation if you please—of the spinal marrow, are found without any of the spinal tenderness. A man was in my office yesterday, with quite a variety of visceral neuralgia, which, according to his story, take turns with a chronic eruption on his skin; there was a fine assortment of the remote effects of spinal irritation, but no flinching when I examined his back. The case was no remarkable one.

Let us try to shun hypothesis, and state what is most important in this connection:—

1. Pains in the course of the anterior branches of spinal nerves, and also spasmodic and various nervous affections, sometimes coexist with pain and tenderness in the corresponding posterior branches; and the tenderness is frequently, as we might expect, at that point where the posterior branch issues from beneath the muscles, to become more superficial.

2. In addition to this, there is sometimes disorder in some internal organ, which is probably primary, the spinal cord serving as the medium through which irritant impressions from this organ are conveyed to the nerves affected.

3. Some persons have, with spinal tenderness and neuralgia, a good deal of suffering in bending, stooping, or even holding so hard as to make the ligamentous stays of the vertebræ tense. I have seen such cases proceeding from a strain, as in trying to hold a runaway horse; and some of them appear to be rheumatic, the ligamentous and adjoining white textures being probably involved.[1]

4. In determining whether a painful affection, say within the abdomen, be inflammatory or not, we may sometimes infer its neuralgic character from the presence of this neuralgic state of the posterior spinal branches.

Beside functional derangements, particularly of the uterus, many organic lesions may produce spinal irritation—as tubercular deposition in its early stages, and wounds of nerves.

As to neuralgia from disorder of the digestive organs, I can say little from my own observation. Sir Charles Bell has given some five cases of tic douloureux successfully treated by croton-oil, and was disposed to infer that they depended on derangement or overloading of these organs, which he easily explained by the connections between the trifacial and sympathetic nerves. Many English physicians of the schools of Hamilton and Abernethy, attach much importance to the treatment by purgatives. It is certain that neuralgic attacks may be excited, in persons subject to them, by indigestible food in the stomach or bowels, and by the passage of cathartic medicines. From the teeth to the rectum, every part of the alimentary canal may make its disturbances felt in this manner. In a case of stricture of the œsophagus, every attempt at swallowing excited acute pain in the little finger. Pain in the lower limbs has been known to come on violently after dinner, and be relieved on vomiting up some ice-cream; also to be immediately removed by swallowing an alkali, by removing fecal accumulations, and by replacing a protruded hemorrhoidal tumour. Many persons of sedentary habits have their tic douloureux usually aggravated by allowing the bowels to become confined, and severe attacks relieved by free evacuation. I have known one who avoided sweetmeats of all kinds, from having observed that they excited paroxysms of facial neuralgia. Possibly, this person had carious teeth, which produced the pains.[2]

The mention of nervous pains in the lower limbs from hemorrhoids naturally reminds us of like cases caused by distension of the bladder and the passage of calculi. I have seen the same after lithotomy. The patient, a man of 70, suffered within less than a week after the operation, from a painful swelling of the thigh and leg, resembling phlegmasia alba dolens, which subsided in five or six days. For a few months after the subsidence of this, he was distressed by fits of lancinating pain down the long saphenous nerve.

Rheumatism undoubtedly depends on a material cause circulating in the blood, probably lactic acid evolved in the transformations of the tissues. It invades all the membranous forms of areolar tissue, from the fibrous textures about the joints to the serous membranes. The sheaths of nerves belong to this class of tissues, and may be the seat of rheumatism. Were I to take my own observation as the only guide, I should be surprised that any doubt of this existed among practical observers, particularly as regards sciatica. This and other forms of neuralgia are common among those who are much

[1] See Wood's Practice, vol. ii.; art. *Spinal Irritation.*

[2] For interesting cases in point, see Edin. Med. and Surg. Journal, April, 1850. (Communication by Dr. Stark.)

exposed to wet and cold and atmospheric changes. Symptoms characteristic of neuralgia not unfrequently supervene in the course of acute rheumatism, toward its close. The analogous affection of temporary paralysis of particular parts, as the hand, without any signs of cerebral affection, has also been observed in the same circumstances. The painful sensations common in persons subject to rheumatism, at times when they are comparatively well, are sometimes indefinite in their seat, but in some cases follow the well-known course of some nerve. Rheumatism attacking the neurilemma, in an acute form, is known by the unusually severe pain, gnawing and piercing sensations, and by its haunting a particular seat, at the same time having marked remissions. I am glad to see that the frequent occurrence of rheumatism in this seat is recognized in the latest excellent work on this subject, that by Fuller of London. The following is the commencement of his chapter, "On Sciatica, and other forms of Neuralgic Rheumatism." "When rheumatism attacks the nerves or their fibrous envelops, it causes pain which follows the course of the nervous trunks, and extends along their several branches. The pain is bounded by such narrow limits, and can be traced so clearly following the track of the larger nerves, that it cannot be mistaken for pain affecting any of the other structures."

Some of the most intelligent clinical observers of our day have pointed out affections of the spinal cord and of the nerves, depending on the gouty diathesis. Dr. Graves has adduced cases tending to show that "gouty inflammation of the nerves and their neurilemma may in process of time extend to the spinal marrow and its investments, and give rise to the derangements of the latter, terminating in ramollissement and structural degeneration." In the *Edinburgh Medical and Surgical Journal*, Jan. 1854, Dr. Begbie gives numerous illustrations of the many forms of gout, among which, besides nephralgia, he mentions three cases of neuralgia in the lower limbs. One, a case of sciatica, was treated with colchicum and neutral salts, and complete relief was quickly obtained on two occasions. Another case, also a sciatica, was of a more asthenic character, the gouty diathesis "more suspected than proved or acknowledged," and yielded to quinia combined with colchicum.

For myself, I have only suspected this connection of neuralgia with gout; in some cases from finding no other ascertainable cause of neuralgic pains; from the hereditary tendency to gout or gravel in a patient's family, and from the good effects of colchicum. The neuralgiæ I have seen under these circumstances were facial (accompanied by inflammation of the articulation of the lower jaw on the same side), and visceral pains somewhat resembling colic.

Syphilis has been pronounced a cause of neuralgia, on account of the occasional cures by mercury. There are few operations of medicine more reliable than the good effects of iodide of potassium in syphilitic headache; and in some cases which were relieved by this medicine, I have been unable, on careful examination, to detect even any commencement of puffy swelling. Were they neuralgic, or are we to assume that there was some brooding inflammation of periosteum or bone? From their lancinating character, and their radiating from a well-known centre of neuralgic pains, near the parietal protuberance, I am disposed to consider them of this nature, in a case lately under my treatment.

The metallic poisons, lead and mercury, by their slow absorption into the blood, produce affections of a neuralgic stamp. The "lead-arthralgia" of Tanquerel is a disease remittent, subject to severe paroxysms, the pain being little dependent on motion of the part, and not increased, but oftener dimi-

nished by pressure. The scalpel shows no correspondent anatomical lesion in the limb, or the nervous centres; lead has been found in the muscles of the limb. The diagnostic marks between this and "common neuralgia," named by Tanquerel, do not characterize a distinct disease. His observations include 1,217 cases of lead colic, 755 of "arthralgia," 127 of paralysis, 72 of encephalopathy. Lead-colic is probably a compound of muscular paralysis and neuralgia, having its seat in the bowels; so that by far the larger number of the cases, where lead poisons the nervous system, partake of a neuralgic character.

Malaria is a well-ascertained cause. Neuralgia prevails in marshy districts and damp climates, and particularly in some of the haunts of fever and ague. A condition resembling the cold stage of intermittents has been noticed at the beginning of the neuralgic paroxysm, marked by a similar character of the pulse, and a sensation of cold, either local or general, followed by local heat. Neuralgia is often periodical, as regularly so as fever and ague, and with the same intervals. The same persons have often had ague and neuralgia alternately; and in particular, many who have suffered first from severe intermittent fever have afterwards had neuralgia. Bark and its preparations, and sometimes arsenic, have rapidly cured these periodical cases.

The following is quoted from Macculloch, in his *Essay on Marsh Fever and Neuralgia* (London, 1828):—

"In this case, the situation was so decidedly subject to malaria, that scarcely an individual, out of many different families which had resided in it, had escaped intermittent at some period of their stay. In one season, and in one family consisting of twelve or fourteen persons, the following were the effects on as many individuals: one tertian, one double quotidian headache, one diseased spleen; in one individual, aged only eighteen, a temporary hemiplegia, with obscure quotidian, and symptoms of diseased spleen; a regular neuralgia of the face, of double tertian type. In a following distant season, and in some of the same persons, there occurred palsy of the face, with imperfect speech—an attack lasting beyond a week, and replaced by quotidian neuralgia (tic); a double tertian, common intermittent, terminating in a quotidian or double tertian neuralgia; a quotidian with neuralgia in the shinbone; the same patient having had, in a preceding season, a common tertian so obscurely marked that he was ordered to Italy for a consumption (a consumption which was cured by two ounces of bark and a change of place to ten miles distance); and, in a following one, having been attacked again with a double tertian, of which one fit was attended by neuralgia of the skin, and the other by a headache."

It is well to mention that the disease is not always of malarious origin when distinctly periodical. Many cases, even coming from a permanent organic cause, recur regularly, with the same intervals as the common forms of intermittent fever.

Lastly, whatever vitiates the blood, by withholding healthy nourishment, or interfering with healthy assimilation, may be the source of this painful disease. Anæmia itself is a most frequent cause of nervous disturbances, both of movement and sensation. They have been observed in limbs after ligature of the main artery, in women blanched by flooding, and in the anæmia of Bright's disease, and chlorosis. Many of these phenomena have been best investigated by Dr. Marshall Hall, to whose *Essay on the Effects of the Loss of Blood*, I may refer.

But the constitution of the blood, and healthy state of the nervous system, may be impaired by unhealthy modes of living as well as by actual disease.

The debility caused by neglect, voluntary or forced, of the great rules of living, is a most common influence predisposing to neuralgia. Our bodies cannot be kept sound without enough suitable food and oxygen, with exercise to quicken the reception of these elements into the system, and to distribute them in due proportion through our organs. The habit of sewing too closely, and the use of such articles of diet as give a temporary unnatural stimulus in the place of substantial nutriment—tea and coffee in particular—are the hygienic errors most needing to be noted.

III. The subject of this essay (as at first announced) requires something to be said of the "nature" of neuralgia. Our knowledge on this question is so slight and unsatisfactory that we shall devote very little space to it. In the majority of cases, where dissection has been had, no morbid changes have been found in the nerves. When found, they have been various, and not always in the part where the symptoms appeared. The knowledge derived from the symptoms and course of the disease is almost equally negative; we learn what neuralgia is not, and some of the laws it observes; but do not know what it commonly is. In truth, while we do not know the nature of the processes by which healthy sensation is effected, how can we understand the mechanism of diseased sensation?

1. Some rare examples of neuralgia depend on inflammation of the nerve. We related a case at page 421, which shows this by its history. In other instances, the nerves have presented redness, serous effusion among their filaments, bloody infiltration, enlargement, hardening, softening, or finally purulent effusion, indicating that inflammation had existed, sometimes acute and at other times chronic. A remarkable instance of the acute kind is mentioned by Gendrin. In this case, and the one just before alluded to, numbness in the parts below the inflamed part was a prominent symptom. We shall presently see that Fuller mentions as signs of effusion in sciatica, "a dull, aching, benumbing pain in the limb, causing it to feel swollen." Simon says: "It is practically of great importance, as a point conducive to accurate diagnosis, to notice whether the neuralgic affection be or be not accompanied by anæsthesia (or numbness.) Attention to this point will often enable us to give confident prognosis as to the issue of a particular case by determining whether the symptoms are such as to imply destructive disease in the nervous apparatus of the part. Neuralgia of long duration can hardly exist separately from anæsthesia, as a result of such disease." Other more obvious signs of inflammatory neuralgia are tenderness on pressure over the whole course of the nerve, felt enlargement of the nerve sometimes observed, and the greater fixedness and constancy of the pains.

But the inflammatory character of this affection is seldom shown, either by its symptoms, the effects of treatment, or *post-mortem* examination. For discussions of this whole subject, we may refer to the able essays of Drs. Oliver W. Holmes[1] and Richard Rowland.[2]

2. In some forms of neuralgia, we may convince ourselves that it arises from a morbid state of the blood, in particular from a poison circulating in it, which we may believe from analogy—perhaps in the case of lead it will be proved by analysis—comes into direct relation with the nerve-substance or neurilemma.

3. We find neuralgia in certain instances a manifestation of disease in the nervous centres; and we may feel that we know something of its nature,

[1] Boylston Prize Essay. Boston, 1838.
[2] A Treatise on Neuralgia. London, 1838.

when we connect it with those laws of the nervous system by which affections of the nerve near its origin are referred to points along its course.

4. Those cases in which disturbances of one organ excite pain in a remote part, through the means of nervous connections, are illustrations of what is called *sympathy*. But this term only serves as a convenient name, like the *x* or *y* of the algebraist, to represent an unknown thing; and though we may study its phenomena, assort them under different heads, and generalize our statements into laws, we are still ignorant of its nature. Interesting methodical accounts of the phenomena of morbid sympathy may be found in the recent works of Henle and Simon on General Pathology.

IV. *Treatment of Neuralgia.*—We have dwelt at length on the conditions under which neuralgia arises, because they afford the most important hints for treatment. Many of them admit of being removed or lightened. It is true that the removal of the apparent causes may not cure the disease, yet even here this knowledge is useful, because the continued operation of the causes may be stopped, and also because powerful nervines have a great control over those cases in which the pains persist after the condition that first excited them has ceased. But there are generally several causes or influences that keep up the disease, and a physician's tact will be best shown in detecting and disentangling them. Thus there is often a general state, such as anæmia or the rheumatic diathesis, making a person liable to nervous pains, and some local exposure or accident determines the place where this constitutional cause shall take effect. If our management be not founded on a comprehensive understanding of the case, its success will be at best very uncertain.

Let us develop these ideas a little in detail. Exposure to cold and wet must be avoided. A person living in a damp and chilly situation, or obliged to work in the water, can hardly expect a cure, unless he can change his business, and go to a dry and comfortable dwelling, or hospital. This applies most strongly to sciatica, yet not exclusively. The best writer on neuralgia of the face, Halliday, urges the same measures, an omission of which may, he says, compromise the good effect of any other treatment. The patient should be taken also from a situation exposed to marsh miasmata. Non-conducting substances should be worn over the affected part, if possible. Flannel and silk are useful, and in sciatica, buckskin drawers. Flannel covered with oiled silk is worn by some, attached to a sort of visor, to prevent, if not to cure, attacks of facial neuralgia. The usefulness of many plasters depends upon their protecting the surface against changes of temperature.

The existence of rheumatism, gout, and the various poisons in the blood, should all occur to the mind as possible causes. We cannot give in full the symptoms or treatment of either of these diseases, yet the following sentences of Fuller are apposite enough to be quoted. They are written with a special reference to sciatica: "If the patient is thin, pale, sallow, and extremely sensitive to atmospheric vicissitudes; if he has experienced pain, or threatenings of pain, in other parts of the body; if, at some former period, he has suffered from rheumatism affecting the joints; and, above all, if his present attack is the result of exposure to cold and damp, the disease under which he is labouring is rheumatic, and is to be relieved by vapour-baths, guaiacum, alkalies, and similar remedies. On the other hand, is he stout, florid, and a free liver, taking little exercise and sleeping much; is he plagued with heartburn, acid eructations, and occasional lowness of spirits; or has he

previously suffered from gout, his malady is certainly of gouty origin, and is to be cured by colchicum, alkalies, and alteratives. Again, is he cachectic, and out of health; has his throat been ulcerated, or his skin disfigured by blotches or eruptions; has he taken mercury, or experienced pain in his bones, the mischief is probably due to a syphilitic taint, and is to be cured by sarsaparilla, with iodide of potassium."

In the pains from syphilis, benefit is also derived from nitric or nitromuriatic acid washes.

When an individual, once strong, has become weak and pallid, without a rheumatic diathesis, and we recognize anemia as a source of the neuralgia, we have a case for the use of iron. The strength and size of pulse, appearance of red blood in the complexion, and presence or absence of other symptoms that are known to be often produced by general anemia, are to be inquired into. Iron is inappropriate, not only in full-blooded persons, but also in those who are predisposed to inflammation or irritation of the stomach and bowels. Its best effects are seen in cases where no local cause is known. Its effects on the action of the bowels are different in different persons. In moderate doses it is apt to constipate, and require some cathartic joined with it. But only a small part of the large doses of the carbonate of iron is absorbed. When a drachm or more is given, most of it passes off in an insoluble form, the sulphuret, and by its mechanical irritation in some persons it promotes peristaltic movement. The large doses, therefore, cannot be more useful than moderate ones, by their increased power of furnishing iron to the blood. Several authors who recommend the carbonate, empirically, insist on the greater efficacy of large doses; and perhaps this is owing to a revulsive action on the bowels. Thirty grains three times a day are often given; some failures are attributed to this *small dose!* It is said that half an ounce every four hours can be well borne, producing no inconvenience unless some feeling of weight in the stomach, as long as the bowels are kept freely open. In employing the carbonate, I have usually given not more than five to ten grains at a dose, combined commonly with powdered rhubarb and an aromatic (cinnamon).

After endeavouring to give some indications for the use of iron in what we may call the *rational* treatment, we must glance at its *empirical* use. Like most other drugs, it has been used too indiscriminately in neuralgia. The subcarbonate of iron was first distinctly brought forward, as a cure for neuralgia, by B. Hutchinson (London, 1822). He reported twenty-seven cases, all but two or three of which were cured, under a course in which this was the leading remedy. In those few, it gave more relief than anything else. Though it has been less uniformly useful in the hands of others, still, it has kept its place as one of the most valuable remedies.

Sir B. Brodie says of the use of iron: "We have never known any benefit to accrue from increasing the dose of the carbonate beyond a drachm thrice daily." He insists on the occasional administration of a purgative to prevent accumulation in the colon.

Whatever the nature of spinal irritation, there is reason to believe it means something, and that this something keeps up neuralgic and other nervous complaints. We must dwell a little on remedies applied over the spine. Simple rubbing and shampooing, even the pressure of a warm hand over the seat of tenderness, and anodyne liniments, give great relief. As a palliative, the following is excellent: one part of solid camphor and chloroform, each, mixed with two parts of olive oil, making a liniment. The belladonna plaster is of more permanent benefit. These harmless measures ought to be tried, unless symptoms are very severe, before leeching and blistering.

After the name of spinal irritation came into vogue in England, the use of leeches and counter-irritation over the tender vertebræ became very common, almost a national peculiarity, which has followed the Anglo-Saxon destiny to this side of the Atlantic. The little book of Teale, written to enforce this doctrine, has gained a reputation in both countries; the edition before me being printed at Concord. It recounts twenty-one cases of external neuralgia, angina pectoris, and other nervous affections, accompanied by some spinal tenderness, and relieved almost constantly by leeches and blisters over the spine. His cases show that these affections may be very much mitigated by such appliances, and some of an acute character even cured. But the long-standing cases were treated by so many different means at the same time, that it is hard to judge how much the local measures aided in a permanent cure, where such was obtained. A great many of these affections occur in weakly persons, who cannot well bear repeated loss of blood, which increases the general irritability. For myself, I have seen *permanent* good come from local depletion only when the cases were very recent. Persons who have suffered long may, however, be relieved of suffering, and, on this account, leeching and cupping may be, on the whole, beneficial in some circumstances. The harsher external applications, according to the best observers, have often failed; I have seen them followed by an increase of spasmodic disturbance, not, that I now remember, of neuralgic suffering.

Sir Benjamin Brodie, in his able lectures on local nervous affections, has raised his commanding voice against the indiscriminate use of these applications, particularly in young and nervous females, in whom we commonly find the symptoms in question. He believes that many are made invalids for life by the repeated losses of blood, each of which may have given relief at the time. He condemns in the same way the practice of enforcing a recumbent posture, and establishing a discharge over the spine, whether by tartarized antimony, caustics, or setons. Good diet, air and exercise, agreeable occupation of the mind, cheerful feelings, with some tonic medicines, are, he says, the best remedies. He also advises the use of belladonna plasters, which I have already spoken of.

From my own limited observation, I am convinced of the truth of Brodie's statement, and that many patients are kept invalids under treatment for "spinal complaint," who would be able to lead a comparatively active and happy life. Have we not all seen such cases? Granting that the condition of a portion of the spinal cord is one of congestion, or too great fulness of blood, is it not a result and indication of weakness? The phenomena of blushing, a result of moral weakness, have been very happily employed to illustrate this kind of congestion, which springs from physical weakness, or from moral and physical combined. To cure it, says an excellent reviewer of Brodie, "we should not leech and blister, or enjoin rest, but we should expose abundantly to fresh air, insist on regular and plentiful exercise, with sufficient intervals of perfect rest, feed with a most generous and nutritious diet, and with but little apprehension, in the majority of cases, of failing in effecting a cure."[1]

Those who consider neuralgia to be very often a result of deranged digestion, will insist much on the management of the digestive organs. I have already mentioned some cures reported by Bell, &c. When an overloaded state of the primæ viæ is suspected, emetics and cathartics may be useful. I have read of remarkable cases where purgatives given for some days, with

[1] British and Foreign Medical Review, July, 1841.

no good effect for awhile, have at last brought away accumulated matters, and given immediate relief. In sciatica, they are often successful. The abdomen should therefore be examined, in any case where constipated habits, with any fetor of breath and loaded tongue, set our suspicions on this track. The following is an outline of treatment which has been very useful in my hands, especially in some cases of dorso-intercostal neuralgia of the left side, where the complexion was muddy, and tongue coated. A blue pill every fifth day, followed by a saline cathartic; extractum taraxaci in pills—say six a day; anodyne applications outside; bitter tonics after pain was removed, and a plain, nutritious diet through the whole period. One case was remarkable for having been aggravated by abstinence from meat or other hearty food, and for the prompt disappearance of symptoms under this treatment.

Dr. Hunt, who gives the most rational advice as to management of the stomach and bowels, ascribes many cases to atony in these viscera, and recommends arsenic after evacuants, to be continued until pain is relieved, and then some tonic to take its place. He combines the arsenic with camphor, or some sedative; or, in case of acid stomach, with bicarbonate of soda, giving small doses of liquor arsenicalis (four minims three times a day at first, increased to *ten.*)[1]

I have nothing interesting to add as to the treatment of other constitutional causes of neuralgia, and will next speak of indications drawn from local causes. Several cases have already been mentioned, where neuralgia was relieved or cured by treatment applied to old scars, &c. A complete removal of the cicatrix by the knife has done this most effectually; and when the scar is tender, pressure on it exciting the lancinating pains, and the situation admits, this is the best remedy. In other situations, as on the scalp, a crucial incision or cauterization may be substituted. A line of cautery carried round the cicatrix will sometimes give temporary relief, and may be advantageously practised at the same time that the cicatrix itself is converted into a slough. This treatment has often succeeded.

"In neuralgia depending on cutaneous eruptions," says Rowland, "relief may often be effected by touching the vesicle or pustule with lunar caustic. In herpes zoster, for instance, I have removed the severe neuralgic pains almost instantly by this means. Sometimes in these cases, one or two vesicles are pointed out as the chief source of the suffering; but, in general, it is better to apply the caustic to the whole of each group, or to cover them with a strong solution of this substance."

Neuroma on superficial nerves is said to have sometimes disappeared spontaneously; and, in some recent cases of an inflammatory nature, arising from injury, leeches are stated (by Frank of Turin) to have succeeded in dispersing it. But it usually presents itself to observation as a chronic disease, curable only by operation. The safest and most eligible mode is to cut away both the tumour and piece of nerve to which it is attached. The tumour is usually too closely implicated with the nerve-fibres to be dissected out. In one instance, where a large tumor was dissected out from the axilla, death ensued, and there was no evident cause for death shown on *post-mortem* examination except inflammation of a nerve.[2]

Neuralgia after amputation is one of the most discouraging varieties. It has returned after three successive amputations of the same limb, and after the sciatic nerve had been exposed and an inch and a half of its length cut out. It has been cured in one instance by acupuncture, and in one by quinia.

[1] Braithwaite's Retrospect, p. 10.

[2] For further directions, consult Mayo's Pathology.

Indications for treatment may be derived from the type of the disease, which may be *inflammatory* or *periodical*. Neuritis may be known by its acute course, by tenderness, perhaps swelling over the nerve, and numbness and thrilling sensations, with little intermission; sometimes by spasms in the muscles, and the presence of some fever. General bleeding is seldom required. Dr. Rowland mentions having procured relief by it in a case of sciatica. Leeches, anodyne fomentations, and poultices, were used with best results in the cases best recorded (those of Martinet). Immersion in warm water, and irrigation with it, as practised in a Boston hospital, have both had very kindly effects. Dr. Holt, in commenting on some cases which he considers inflammatory, says: "Cold applications in these cases are commonly agreeable and beneficial, as a means of relief. The application of ice, or cloths wet in cold water, is often a very valuable auxiliary in this form of the disease.[1]

The writer on rheumatism, Fuller, from whom we have already drawn quite freely, recommends means particularly to meet the condition of effusion within the sheath of the (sciatic) nerve. The signs of this effusion he thus enumerates: "When a patient who is suffering from sciatica complains of a *dull, aching, benumbing pain in the limb, causing it to feel swollen;* when this sense of numbness and increased bulk has succeeded to pain of greater intensity, accompanied by cramps and startings of the limb; and more especially when, in addition to these symptoms, there is more or less inability to move the limb, inability arising from loss of power, and not as a result of excessive pain," &c. In these circumstances, he recommends that measures be taken for the specific purposes of checking further effusion, promoting the absorption of that which exists, or mechanically evacuating it. For these purposes he uses cupping and leeching, if the effusion is supposed to be recent. In cases of longer standing, he uses long and narrow blisters along the track of the nerve, dressed with mercurial ointment, or sprinkled, if necessary, with powder of pure morphia, also mercury internally. In somewhat obstinate cases, when inflammation has gone by, and only its result, effusion, exists, he recommends acupuncturation, to *mechanically evacuate* the fluid. "Six or eight pairs of needles, specially adapted for the purpose, are carefully inserted into the thigh, along the course of the sciatic nerve, the object being to puncture the neurilemma, and thus to allow of the escape of the fluid." This operation he has seen give complete relief in three cases, and in one the relief was permanent. He thinks failure may have been owing to the operation being tried when there was no effusion, or when inflammation was not subdued, or to the fluid not being reached.

Acupuncture has been used without this definite indication. Cloquet reported many successes from it, the needles being left in for some minutes, or even hours. Drs. Renton and Elliotson, in England, also used acupuncturation extensively, with benefit, in sciatica. The former left them in only five or ten minutes, inserting sometimes ten along the thigh and leg. He effected many immediate cures in old obstinate cases. He inserted the needles with a slight rotating motion, and "*without pain.*"[2] The effects of acupuncturation, employed *empirically*, have been, on the whole, quite uncertain.

When neuralgia is *periodical*, following any of the common types of intermittent fever, whether caused or not by miasmata, it is usually cured for some months, if not permanently, by preparations of bark.

Even where the attack is not exactly regular, but tends to return about the same part of the day, there is no one medicine more to be relied upon, in the neuralgia so common in females of weak constitution and sedentary habits.

[1] Bost. Med. and Surg. Journ. May, 1845. [2] Lond. Med. Gaz. July, 1841.

Many patients form a habit of using quinia on the return of a paroxysm of tic douloureux, cutting it off speedily, and relieving them for several months, or even a year or more. Some physicians employ grain doses three or more times in a day. Some, who have gained a good deal of local reputation, and self-confidence in the treatment of facial neuralgia, administer ten or fifteen grains at a dose. I have known the small doses to succeed immediately after the large had repeatedly failed.

Brodie says that very large doses may be needed. He had to raise the dose, in one instance, up to half a drachm daily, of something called quinia. It is used in as large quantities as that in France. Brodie speaks with perfect confidence of succeeding by the use of cinchona or quinia, combined, if needful, with

Arsenic. This and the carbonate of iron are both considered particularly useful in periodical neuralgia. Having no extensive experience in its use, I will once more quote Dr. Hunt: "Arsenic operates most favourably on persons who are of lax fibre, accompanied by a languid state of the circulation, and whose secretions are rather profuse than otherwise; the urine pale and plentiful, and more especially on those whose skin is cold and moist. In persons of this description, whilst arsenic, to an extent far beyond other medicines, relieves the neuralgic pain, it improves the general health, and gives firmness and vigour to the constitution. When the urine is of a deep colour and scantily secreted, or when it deposits the lithate of ammonia, the tongue loaded, especially if the tip or edges of it are red, arsenic almost invariably disagrees, and aggravates the pain. But as this morbid state of the system frequently depends on or is complicated with disorder of some important viscus, arsenic will often agree, and relieve the neuralgic pain, after the visceral disorder has been removed by appropriate remedies."[1]

Our distinguished countryman, Dr. Gross, has found Fowler's solution less serviceable than the arsenious acid in substance—less effective in curing the disease, and more likely to nauseate.

In some intermittent cases, M. Cazenave applied ligatures or tourniquets to the limbs, with very happy effects. One patient had excessive pains in the head, returning periodically, and baffling all treatment. Ligatures were applied to each limb a few hours before the expected attack, moderately tight. The pains were lessened the first day, still less the next, and did not appear at all on the third day. He advises that the application be continued after the disappearance of the neuralgia, for five or six days.

But supposing that we have no obvious indications for treatment, drawn from causes of disease to be counteracted, or from the peculiar features and type of the case in hand. We have still many resources, and some light—alas! too little—to guide us in their use. It will be necessary to name a good many medicines that have gained the best reputation in this painful disease. They are undoubtedly used too much at random; and what we need is, less to have new medicines, than to learn how to use what we have. Let me once more enjoy the luxury of transferring to my humble pages the counsels of one of the most judicious, observing, and enlightened physicians, Brodie. He is addressing students.

"Nor can I think well," says he, "of this modern fashion of resorting on all occasions to novel methods of treatment. I advise you, if you wish to succeed in your profession, and to be useful to society, to pursue a different course. Make yourselves masters of the old remedies. Learn how to handle them, and what good they will do, and, as a general rule, have recourse to

[1] Prov. Med. Journ. April, 1844.

them in the first instance. If the old remedies fail, and you are at a loss as to what you should do, then, and not till then, have recourse to the new ones."

In estimating the value of any medication for this disease, we must remember that intervals of ease, extending over a period sometimes of several months, may give us a hope that the disease is finally cured, but the pains may afterwards return. Many modes of treatment are successful only up to this point, and in the most exquisite forms of neuralgia it is a blessed boon to obtain even this respite.

The medicines which have done most service by their employment internally, might be arranged, not very strictly, however, under the general heads of sedative or narcotic medicines, tonics, and medicines whose operation is not understood—unclassifiable, alterative medicines.

As mere palliatives, sedatives are useful by cutting off the paroxysm, giving relief to pain, and lessening the power of that unhappy habit which makes future paroxysms more likely to ensue. In this respect, the anæsthetic vapours appear to me to be more useful than any other substances. Believing that chloroform is unsafe in the best hands, I always prefer sulphuric ether, though much less convenient and elegant. It need not be inhaled long enough to make one insensible; the power of perceiving pain being lost before consciousness of identity of perception of surrounding persons and objects. In forms of neuralgia depending on mere nervousness and irritability, the pains will not always return as the effects of ether disappear, and inhalation, therefore, does not require to be continued long. Many interesting facts of this kind are related by English physicians.

These substances are all more applicable to neuralgia excited by irritation reflected from a distant organ, or some temporary exposure, than to those cases where some local mechanical cause affects the nerve itself.

With a curative object, the best effects from sedative medicines are got by giving some of the vegetable narcotics, in full and frequent doses, until the pain is quelled, then continuing them with larger intervals for a while, and finally substituting a tonic. Among these narcotics, it is difficult to say which has the greatest weight of testimony in its favour. Leaving out aconite, as being a substance of very peculiar properties, we find different practitioners preferring belladonna or conium—less generally, stramonium or hyoscyamus, or the preparations of opium.

I have collated the testimony of many authors whose communications appear in the journals, as well as the systematic writers on neuralgia; but as the same principles govern the administration of all these remedies, I will not attempt to give the various facts on which different practical men found their preference for this or that medicine. Belladonna, employed on the principles just named, has, I think, the strongest testimonials in its behalf. The following mode of employment has succeeded very well with M. Trousseau: A quarter of a grain of the extract is given, in pill, every hour, until some dizziness or effect on vision is produced, by which time the pains are quieted. Then the medicine is continued with longer intervals. This course must be resumed, if the pains recur. After they have disappeared, a tonic is used; usually quinia or iron. This plan succeeds best in facial neuralgia. According to all who have used it, belladonna must be continued until some slight effect is perceived in the throat or state of vision, or vertigo. In full-blooded persons, it may be combined with ipecac., as recommended by Golding Bird for neuralgic dysmenorrhœa; or, in those who are pale and feeble, with iron or zinc.

Conium is preferred by many. Mr. Lawrence says it is the only one of these narcotics on which we can have any reliance for checking the paroxysms.

"I have seen in several cases, where it has been given largely and at short intervals—and it must be given in such doses as to produce some of its peculiar effects on the nervous system—that it has put a stop to the paroxysms, and for such a length of time that I have thought it had cured the disease; but in some instances, where persons have remained well for several months, and even in one case for more than a year, the pains have again showed themselves; but the agony has been considerably controlled by it." He does not mention what has been the result of following up this temporary cure with a tonic course.

Such has been the experience with all these narcotics. To state who has succeeded best with one, and who with another, would be hardly a profitable use of our remaining space. It is necessary, and often difficult to procure the substances pure and strong, especially the extracts. We must also seek carefully for any circumstances that may aggravate or keep up the irritation, and attend to the state of the bowels, and several secretions.

Opium and its preparations, are considered to be better suited to internal than external pains. Morphia and its salts are most employed. Their use may be profitably confined, I think, to cases that recur at night, or early morning, when an evening anodyne may be extremely useful. The acetate of morphia is stated, however, to have produced some cures, in the hands of Dr. Bardsley and others. But the more common opinion is, that it tends to increase nervous irritability and costiveness, more than the other narcotics, without having so much control over neuralgic pain. For immediate relief to agonizing paroxysms, it is every way inferior to sulphuric ether, used by inhalation.

Aconite.—The internal administration of aconite has often disappointed physicians, yet when used in full doses, and continued for some time, permanent relief has often been derived from it. Dr. Fleming has called particular attention to its power of depressing nervous and vascular action, and to its use in neuralgia. "He gives a table of forty cases; ten of these have been published by others, and they are all permanent cures. Of the thirty cases which fell under his own care, there were seventeen permanent cures, and in thirteen the relief was only temporary. In some of these, the medicine was used internally, in others externally, sometimes both." Aconite is considered an antiphlogistic, not well adapted for feeble persons. Dr. Fleming suggests that its internal use is most likely to be beneficial, when neuralgia depends on inflammation in any part of the nerve (a condition quite unusual), or on sympathetic irritation; its external use, when the disease arises from local functional irritation. The strong tincture which he recommends has occasioned several deaths, by its accidentally being swallowed in an overdose. He uses five minims three times a day, increasing cautiously. The article we get here is apt to be quite weak. On the whole, the results of aconite used internally, as I find them reported, do not seem to me so favourable as those of belladonna, while it is much less safe and manageable.

I have obtained good effects from the following combination of aconite with tonics. ℞.—Iron by hydrogen ʒij; tinct. aconit. rad. (sat.) ʒj; M. ft. pil. lx. Give one pill after each meal, until the neuralgic pain ceases, and then substitute the following tonic pills. ℞.—Quin. sulph., iron by hydrogen, āā ʒj; M. ft. pil. lx.

All these narcotics, including aconite, have been known to cure obstinate cases, when given by accident in excessive quantities. I have collected from different sources a number of cures, said to be permanent, gained by means of narcotism produced suddenly by large doses of belladonna, opium, and aconite. The same thing has happened with strychnia. A lady who had suffered

severely for years, from facial and cervical neuralgia, took by mistake a teaspoonful of the strongest tincture of aconite root, which had been prepared for use by friction. The immediate effects were most distressing and dangerous, her life being saved only by the prompt employment of stimulants. After her recovery she continued wholly free from the neuralgia for more than a year, after which time it returned, in a milder form.

One Dr. Chabert, of New York, proposes to only poison his patients with prussic acid, not quite dead, and then bring them to. He succeeded in one instance, if we may believe him (not only in bringing the man to, but in curing the neuralgia.) These results are curious, whether practically valuable or not.

Nux vomica and strychnia are medicines of great utility in the neuralgia of feeble persons. The former was long ago recommended by Linnæus as a cure for gastralgia. The cases it is believed to be best fitted for are those occurring in pallid, lymphatic persons, not those subject to derangement of the motor functions. Rowland thinks strychnia most valuable in intermittent neuralgia. It acts as a tonic, particularly promoting the digestion of food. It can never be administered without anxiety, and should be closely watched. I have known myself of but one instance of its having poisonous effects, when given for neuralgia. But whether from a cumulative property, or from varying susceptibilities of the system, it will sometimes break out unexpectedly, as in the case I alluded to. The first spasmodic symptoms were violent trismus, with a sense of suffocation, &c. The patient was saved, and the gastralgia she had been long suffering from, was almost perfectly cured. I suspected its employment in homœopathic practice, from tetanoid symptoms connected with some mysterious pills, in one young lady, whose disease was, however, a form of chorea.

In the *Medical Gazette*, 1840, Dr. Pidduck relates his experience with strychnia, which is important. He gives six cases, two of infra-orbitar neuralgia, one sciatic, one in the arm and hand, accompanied by *loss of power*. The strychnia was given usually one-twelfth of a grain twice or three times a day, and always with benefit. In one case, relief began after the third dose, and was complete before the twelfth; the pain returned in slighter degree a few days after, and the medicine was resumed with equally good effect, after which there was no relapse. When any mechanical irritation exists, at any part of the nerve, it is not likely to do good. If I understand Dr. Pidduck rightly, he ascribes much or all of the power of strychnia over paralytic and painful diseases, to its tonic properties, and its producing more complete assimilation of food. From all the reports of strychnia in neuralgia, I should infer that it was a medicine promising much good, and particularly deserving of further trial.

Among tonics, having already spoken of the two most important, iron and quinia, I need only allude to the employment of zinc. The preparations of zinc are believed to be valuable tonics, more particularly in affections of the nervous power of motion, as chorea. The pills of Méglin, celebrated in France as a remedy for facial neuralgia, consist of equal parts of extract of hyoscyamus, extract of valerian, and oxide of zinc. How much the zinc adds to their efficacy, I have no means of knowing; though it appears that the zinc is sometimes omitted.

The valerianate of zinc is a preparation of great value. It combines a soothing with a tonic power, and has, so far as I have observed, no tendency, like so many of the sedative drugs, to derange the secretions and impair digestion. It is found most beneficial in facial neuralgia, not presenting a regular periodical type, and not connected with rheumatism or syphilis. I have before

me the notes of three cases, in which its good effects were obvious, after many other powerful remedies had failed. I am in the habit of administering three-quarters of a grain, in pill, two or three times a day.

Turpentine has succeeded so often, in sciatica especially, that it ought, I think, to be ranked next in value to the carbonate of iron and quinia, if any general comparison could be instituted between remedies all adapted for different varieties of neuralgic disease. Of all the testimony and advice in regard to it, none is so important as that of Martinet. He settled down on a preference for doses of about twenty drops, three times a day, allowing of its absorption. When it produces diarrhœa, he finds its good influence thwarted. It produces a strong sense of heat in the affected part, as well as in the stomach and bowels, sometimes a general perspiration. This sense of heat in the part is most marked in those who are cured or relieved.

"This medicine is of approved efficacy," says one of the notices of Martinet's essay, "in all cases of neuralgia affecting the extremities, and particularly in sciatica, when this disease is simple in its character, and evinces no sign that the nerve is either altered in its structure, or compressed by the formation of a contiguous tumour. M. Martinet affirms that, whether the complaint be recent or otherwise, the chance of cure by this remedy is greatest, *cæteris paribus*, when the pain is so intense as to indicate distinctly the course of the nerve, and so obstinate in its nature as to yield to no other treatment whatever." "Twelve days," he says, "usually suffice for curing neuralgia, when it affects the extremities, and more commonly only half that time. To continue it too long would only injure the organs of digestion, if not the urinary organs."

Of seventy individuals, affected chiefly with sciatica and other kinds of neuralgia of the extremities, fifty-eight were cured, viz: three by frictions, and all the others by taking it internally; ten (two of whom intermitted the medicine) obtained only temporary relief from its use, and five received no benefit. Of these seventy-one cases of neuralgia (for one of the patients had two affections of the kind) forty were acute, and thirty-one chronic. Of the acute, thirty-four cases were cured, five relieved, and only one continued in the same state. Of the chronic, twenty-four were cured, three relieved, and four received no benefit.

Dr. Copland speaks highly of the use of turpentine in other neuralgiæ besides sciatica. He says: "A strong recommendation of it is found in its being equally appropriate to the inflammatory and non-inflammatory states of the affection, and in the fact of relapses being much less frequent after its use than after any other remedy."

Colchicum and croton oil may both be considered as having more power over some forms of neuralgia than they owe to their mere effect as evacuants. Both act at once on the kidneys and the bowels, but they have a curative influence beyond this. Croton oil, as has been observed both by Andral and English physicians, has produced cures when given so as to have no more cathartic effect than purgatives that had been previously used in the same cases. As it has been proved that colchicum often increases the proportion of lithic acid and the lithates in the urine, and its best effects are observed in gouty neuralgia, we may infer that it acts by promoting the excretion of these elements from the blood. Croton oil also increases the density of the urine; what solid ingredients it acts upon particularly, I do not know. Professor Easton suggests some analogy as probably existing in the curative agency of these two substances.[1] This agency has not won such general confidence as many of the medicines before named.

[1] London Lancet, 1842.

External applications form an important part of the resources for treatment. Some of those which affect in the simplest manner the circulation and nervous activity near the skin, are very useful as palliatives. Pressure and rubbing are often employed instinctively. We have stated that, generally, pressure on certain radiating or focal points aggravates the pain. But in ordinary cases, not inflammatory, pressure bearing pretty evenly along the whole course of a nerve relieves it. I have more than once completely stopped a paroxysm of cubito-digital neuralgia by careful bandaging of the arm. The pain is often pressed away to another spot, and I have succeeded in thus chasing it off from the fingers' ends.

Both cold and warm applications have been found excellent palliatives, in different cases; just as with weak eyes and some forms of chronic inflammation, it can only be determined which will be most soothing, by actual trial. Compresses with cold water and ice; douches of cold water have sometimes carried off paroxysms.

Dr. Warren applies very frequently warm and hot fomentations; and the hot douche. For the latter he has a vessel of hot water, a stopcock and hose, and lets on a stream of water, say a quarter of an inch in diameter, at a temperature of 110° F., continuing it as long as the patient can bear it, which is usually for five or ten minutes. Then he applies frictions with olive or palm oil for a few minutes. He has met with much success from these applications. It is only in the severer cases that he adds to hot fomentations some narcotic plant, as conium, hyoscyamus, or hops.

Rubefacients—the ginger or mustard poultice—deserve their reputation in tic douloureux. I have seen a paroxysm completely cut short by a weak solution of ammonia applied to the cheek, producing most abundant flow of tears.

Blisters are among the most valuable remedies. Dr. Warren says they are perhaps the most frequently useful of all. They should not be kept open long, or allowed to run into suppuration, but fresh ones should be applied from time to time. Two modes of application have been most successful. One is, by placing rather long blisters along the track of the nerve, and the other consists in putting small repeated ones over the uppermost point affected, or that which serves as the radiating centre of pain. It is in this respect that Valleix thinks his investigation of the "focal points" of neuralgia will be useful in guiding our treatment. He relates a number of cases, some of them highly interesting and striking in their results, in which a succession of small blisters over these points was employed. The plan is stated to have produced a cure in many cases, and to have always given great relief. Blisters are also very beneficial in inflammatory cases, after the acute stage has passed. Fuller recommends them, when symptoms of effusion exist, the active signs of inflammation having disappeared—and advises long and narrow blisters, along the course of the nerve, to be dressed with mercurial ointment, to aid in obtaining the constitutional effects of mercury, or with powdered morphia, to relieve severe suffering.

In the obstinate affections, sciatica especially, issues and the moxa at times succeed, when nothing else will. A distinguished physician of this city relies mainly on issues made with Vienna paste, over the upper part of the ischiatic nerves in obstinate sciatica. When the disease shows a disposition to change place from a nerve it has long occupied, we may favour this disposition by making an issue on the part it seems inclined to invade. This is illustrated by an interesting case in Dr. Rowland's Essay, where facial neuralgia of the severest kind had lasted nearly twenty years, when the pain showed an inclination to attack the shoulder, and an issue applied there cured, almost com-

pletely, for two years at any rate, the excruciating affection of the face, while a slight neuralgia persisted in the new seat.

Many of the sedatives are used externally with even more advantage than internally. We have already alluded to the fomentations of hyoscyamus, &c., which have often been very beneficial in the hands of Dr. Warren—and also to the use of belladonna in plaster, over the spot of spinal tenderness. This last I have also found exceedingly serviceable in many simple neuralgiæ, as also chloroform, by itself or combined with camphor in a liniment.[1] In many country districts, where belladonna grows abundantly, poultices made by boiling down together the roots and leaves of the fresh plant, have attained much celebrity, both in rheumatic and neuralgic affections. The narcotic effects often follow this mode of employment. We are assured by a physician of such a region, that this treatment is often successful without any internal medicine. Permanent cures are effected, within a fortnight, of facial neuralgia that had existed for years. Ointments and watery solutions of belladonna are reported, by Dr. De Brayne, Dr. Todd, and others, as having been successful in producing permanent cures. Dr. Lombard has given very satisfactory results from the use of lotions of hydrocyanic acid and its compounds; in neuralgia of the more superficial nerves, not complicated by inflammation.

Aconite, its saturated tincture, and alkaloid, possess an extraordinary benumbing power, substituting for acute pain some disagreeable tingling sensations. In my own observation, I have not found them so useful palliatives as chloroform, and can judge of any curative power they may have only from books, which give cases treated by so many other means at the same time as to have little real scientific or practical value. In some of Dr. Fleming's reports, the drug was only used in this mode. One crural neuralgia, confined to a spot near the patella, had existed three years, and been relieved by nothing but the potential cautery; it was cured in eight days by the external application of the tincture of aconite. This application has sometimes produced the general symptoms of poisoning by aconite, and should be used with great care, particularly if there be any abrasion of the skin.

Of both aconite and veratria, the results have not been near as favourable in other hands as they were represented by Turnbull, and some others who first brought them most prominently forward. Pereira, Brodie, Rowland, all agree in this statement. Pereira found the veratria generally fail, and considered it far inferior to aconite. Brodie employed it in several cases without any advantage. Rowland never obtained permanent relief from it in any case of long standing. He says the first friction with an ointment of veratria often gives very great relief; perhaps this continues a good deal longer than the usual intervals of ease, but the pain keeps recurring and the intervals of ease keep shortening, until they hardly outlast the unpleasant prickling occasioned immediately by the application. Atropia, the alkaloid of belladonna, has cured an obstinate facial neuralgia, an ointment of five grains in three drachms of lard being rubbed in a small piece as large as a pea, three times a day.

The endermic application of narcotic and anodyne substances deserves to be more used than I have ever known it to be in this country. A blister should be raised, better by ammonia than cantharides, and the exuded coagulable matters should be removed at each application of the drug, since they would interfere with absorption if allowed to remain. The substance applied should be in the form of powder, or perhaps watery solution, not cerate. Fuller mentions that the salts of morphia give much more pain than pure powder of

[1] Cazenave employs a "pommade," containing chloroform ʒiv, cyanide of potassium ʒiiiss, lard ʒiij.

morphia. The acetate, however, has been most employed. The place of application should be well chosen—the same as in the ordinary use of blisters for neuralgia, along the track of the nerve, and at a point where this nerve is superficial, and especially one of the *focal points.*

M. Magistel published his results obtained by the endermic use of acetate of morphia, about twenty years ago. His were cases of hemicrania and neuralgia of the face. His success was great, but Trousseau since thought he gained great advantage by substituting the more soluble salts, the sulphate and muriate.

An interesting paper in the *Lancet*, 1846, contains some successful cases, treated by Dr. White, of Newcastle. The following are some remarks of the reporter:—

"This mode of treating pains which appear to depend principally upon nervous irritation, or it may be chronic inflammation, and are confined in their extent, has been extensively employed here, and in no cases has it been attended with any disagreeable effect; but, on the contrary, with the most surprising benefit in many, or, I might say, in most of those patients who, from their condition, seemed to be suitable for its application. The pain is generally soon relieved, and a comfortable feeling of ease, with sometimes a little drowsiness, is experienced. In fact, in sciatica, or indeed in any case where the pain can be distinctly traced along the nerve, this plan is often the only one that affords even partial relief. The application must be used as near the trunk of the nerve as possible, particularly where the pain most nearly centralizes. It sometimes requires to be reapplied nearer and nearer the terminating branches, as the pain is gradually expelled. Hence, in sciatica, the nates first, then the knee, and lastly the foot, sometimes in succession, require the vesication, and subsequent sprinkling of the morphia. It is quite certain that this treatment has entirely succeeded when leeches, blisters, moxas, belladonna externally, &c., had entirely failed."

I regret that the interesting narrations now before me, of old cases of neuralgia in the sciatic and trifacial nerves, treated with great sagacity and happy results, and very well reported, by M. Mondière, cannot be copied into this essay, with their suggestive details. Some of them were of the most exquisitely acute form; some had tortured the patients for years, and resisted the greatest variety of scientific treatment; and relief was obtained in two or three applications, increased day by day, and the results as to permanent cure were most happy. His cases were published earlier than the investigations of Valleix; and it is interesting to observe that his best successes were obtained by vesication and the sprinkling of morphia over points, exactly such as this later observer has indicated.[1] In these cases no internal treatment seems to have been employed at the same time, though the cures were afterwards followed up and confirmed by tonics, or other management as required. The inoculation of solutions or pastes containing morphia has very often failed, and deserves to be mentioned rather for curiosity. Its practical advantages over the application to a blistered surface do not appear, unless in a slight saving of pain, while its success is much less. Dr. Rowland obtained good results from it in but one case, out of "at least twenty." But I have been much impressed with the success of the "endermic" treatment as above described; and cannot but think it more deserving of trial than almost any other measure we have been considering, which is at all novel.

After reading a good many papers, on the use of electricity and galvanism in neuralgia, the result is that I am a good deal disappointed in the accounts

[1] Répertoire de Clinique, &c. 1836.

of their efficacy as means of cure. In the *London* and *Edinburgh Journals* are some communications exhibiting facts of interest. M. C. James attributes the failures to the imperfect application—recommending electro-*puncture*, the needle (of platina or gold, and fine) carried to the nerve itself, not necessarily deep enough to transfix it. Needles are inserted near the origin, and near the end, of the affected nerve, and a current passed between them from a voltaic pile of say five or six plates, continued for a few seconds only, the positive pole being made to communicate with the needle towards the origin of the nerve.

M. Hermel has reported cases of cure by electro-puncture, almost all of lumbo-sacral and sciatic neuralgia, sometimes accompanied by partial paralysis. His conclusions are thus stated:[1] "1. Electro-puncture is applicable to idiopathic or essential neuralgias. 2. The violence of the pains is not a counter-indication to the employment of this therapeutic agent; they have never in any case been aggravated by its use.[2] 3. The paralysis which supervenes in the progress of idiopathic neuralgias yields to the same treatment." It may be mentioned, for the gratification of our homœopathic friends, that Halliday knew of a case aggravated by it, and Bérard, of Paris, gave himself a neuralgia by experimenting with acupuncture for the purpose of applying electricity.

Some successes have been obtained by the electric spark and by galvanism. The latter has been used by Dr. Thomas Harris, with the result of curing five out of seven cases, but only after intervals of from ten days to five weeks. Among various affections which I have known to be somewhat relieved by the use of galvanic rings, was one of sciatica, of thirty years' standing. The relief coincided with the appearance of an eruption of an eczematous character, which spread from the point of application down to the leg, and was more troublesome than the sciatica itself, while it lasted. After four or five weeks the eruption was nearly healed and the sciatica returned, but much mitigated in severity. I am also acquainted with ladies who count confidently upon cutting off an attack of neuralgia by applications of the wires connected with the galvanic pile. Their frequent resort to it shows that it has no curative powers for them.

The effects of all these agencies are not so well understood that we can safely foretell their operation in any particular case. Empirically employed, with the slight indications we can gather from our imperfect knowledge, their workings appear to be capricious. The words of Pereira, applied to the magnet, have too much truth as an expression of our experience with these other powers. "In some instances it has appeared to exercise a most remarkable influence over neuralgic pains and spasmodic affections; at one time apparently curing, at another palliating, and occasionally augmenting all the patient's sufferings. But, in a large proportion of cases, it fails to produce any obvious effect."

It remains to speak of surgical operations for neuralgia, and we shall do it briefly. Besides operations already spoken of as called for by the known existence of a scar or other local source of irritation, surgery has been called to render her aid empirically, in cases which resist other modes of treatment. Three principal operative procedures have been performed, all with occasional success, namely, section of the nerve, excision of a part of its length, and cauterization. Mere division of the nerve generally gives ease only for a time, pain returning in a few days sometimes, oftener a few weeks. The case of Dr. Haighton, who performed this operation on his own mother, is commonly cited among the instances of success; but it appears that the pains reappeared

[1] Quoted from Velpeau, in Copland's Dict. [2] A mistake.

after a long time.[1] Cases of reported success by this mode are numerous, but those of failure far outnumber them.

Excision and cauterization count a much larger proportion of favourable results. Dr. Warren has frequently practised the former on parts of the trifacial nerve. He exposes the nerve, passes a curved knife under it, raises it and cuts out half an inch or so of its length. He then lets the wound heal immediately. He believes that this operation would be generally successful, if performed within a few weeks of the origin of the disease. It should be done, in his opinion, as soon as other modes of treatment have been tried and have failed. By longer delay, "the organic habit" of pain is set up, and the whole course of the nerve becomes diseased. He has observed several instances where the paroxysms did not disappear at once, but the pain abated only slightly for a while, and then either continued to diminish gradually, or else disappeared suddenly, after a time. "After the paroxysms have been cured," says he, "exposure to cold on the occurrence of an injury has reproduced them in the same or in neighbouring nerves."

Cauterization has been thoroughly accomplished by first cutting the nerve across, and then touching the ends with nitrate of silver, or a small heated iron. It is considered by Boyer the most effectual operation of all, as "it destroys not only the whole substance of the nerve for a certain distance, but it also attacks all the nervous branches proceeding from it through a considerable extent, which as they may participate in the disease, might be sufficient to keep up the pain, after the resection of the principal trunk." In the detailed accounts of cases, cauterization is not mentioned as having been followed by the same partial relapses and remnants of disease as Warren has observed after excision. In one case of final success, however, dangerous general symptoms, convulsions, &c., occurred.

The successes of either excision or cauterization surpass those of mere section of the nerve. Between the first two, the choice must be determined, not so much by any general law, as by the convenience of each case; in particular the depth and size of the nerve, and the importance of avoiding deformity in the situation it happens to occupy. The difficulty of finding and isolating the nerve, so as to be sure of dividing it, and nothing else with it, is spoken of by Boyer as very great, and a serious objection to the operation by excision.

It seemed to me more convenient, though less logical, to consider the different modes of operating first, and the question of the expediency of any operation afterwards, inverting the order which we should follow in practice. It is certain that comparatively slight operations have sometimes gained long respite, in a disease which is one of the most painful we are acquainted with. It is a disease which has led several sufferers to submit, for the uncertain chances of relief, to repeated amputations in the large joints, even when the disease had first originated from amputation, and before anæsthesia had charmed away the shadows of fear and agony from the atmosphere of the operating-room. On the other hand, we know that many forms of neuralgia tend to travel from one nerve to another, and seem more disposed to invade a new region when they are dislodged from an old haunt; that many spring from a constitutional cause; that others depend on disease in the nervous centres, the presence or absence of which we cannot always ascertain; and that in the limbs we have motor and sensitive fibres in the same nervous trunk, not separable in an operation, so that a permanent loss of function paralyzes the muscles it supplies, and would cause the greatest inconvenience, if it occurred in the main nerve

[1] Rowland, p. 90.

of a limb. Yet the sciatic nerve has been cut down upon by Malagodi, and eighteen lines in length of it were cut out, with the effect of leaving a paralysis of the leg and foot, instead of the neuralgia.

To show that there is room to question in a general way the utility of operations for neuralgia, let me once more refer to the highest surgical authorities. Dr. Mott's views are stated as follows by Reese, in his *Notes to Cooper's Surgical Dictionary*, 1839:—

"Dr. Mott has adopted the practice of dividing the nerve in almost every case of neuralgia, where it is practicable. He has repeated this operation, on the infra-orbitary, mental, and other nerves, so frequently and with so great success that he confidently recommends it to his pupils and patients." In Dr. Lee's additions to *Copland's Dictionary of Practical Medicine*, 1847, is a sentence apparently referring to Dr. Mott's opinions expressed in his *Lectures on Neuralgia*, 1842, and containing these words: "After extensive trials he laid the operation aside, as he found the disease very certain to return as severe as ever."

Brodie says of the operation for facial neuralgia: "It is altogether an unscientific operation, from which we have no more right to expect benefit than we should have from the amputation of the testis, in a case of pain referred to that organ, in consequence of a calculus being lodged in the urethra." We may urge in reply: 1. That the analogy does not hold between an irritation temporarily reflected from one organ to another, and a fixed affection, always haunting one particular nerve. 2. That the operations have frequently succeeded, at any rate, in obtaining a welcome respite of months or years.

Many points pertaining to the general management, or regimen of neuralgic patients have come up incidentally in this essay. Their modes of living ought in all cases to be regular, the diet simple, generally nutritious and compact, except in cases of plethora or inflammation. Abstinence from tea and coffee ought always to be enforced, at least long enough to test its effect. The habitual use of these nervines keeps up a morbid sensitiveness of the nervous system. I have observed this particularly in regard to strong tea used by feeble females. With some of this class, it is unfortunately impossible to avoid a confining and toilsome life, often in confined air and constrained position, bent over the monotonous labours of the needle. Thoughtfulness and kindness on the part of employers must here be looked for to accomplish what science can only indicate. An early hour of going to bed and rising should be observed. The mind should be preserved from overstraining and from trying excitements. Worrying, and particularly angry feeling, are a frequent exciting cause of paroxysms. Regular exercise of the different muscles should be insured, as systematically as is consistent with cheerful employment of the mind and feelings. A few cases have been cured by exchanging an inactive life for one of fatigue. Pinel mentions a gentleman cured at last of sciatica, by becoming a soldier. Another patient got rid of a facial neuralgia by aid of the practice of boxing, carried often to the point of great fatigue.

In quoting often from writers of reputation, I have been influenced only by a desire to convey, in the simplest and surest way, the results of varied observation. When men of sagacity and large experience have given us their views, in regard to the disease in hand, or to the several collateral subjects, which each man has studied separately more thoroughly than one man can study all, we may well borrow their statements, not indeed to adopt them blindly, but with all the deference due to practical knowledge and mature thought.

REVIEW.

ART. XIV.—*Trial of John Hendrickson, Jr., for the Murder of his Wife by Poisoning, at Bethlehem, Albany County, N. Y., March* 6, 1853. Reported by Messrs. BARNES & HEVENOR. Albany, 1853. 8vo. pp. 176.

THIS report contains the history of one of the most interesting and important trials in the records of medical jurisprudence in our country. It is the first, and we believe the only instance in which the question of poisoning by *aconite* has been brought before our courts of law; and the facts elicited on the trial, though of a negative rather than a positive kind, are of great importance in legal medicine, and deserving of permanent record.

The present report has been published under the sanction of the counsel who were engaged both in the prosecution and defence of the deceased, and may therefore be regarded as a correct and authentic document. It contains, in addition to the testimony elicited upon the trial, the arguments of the counsel, the charge of the judge to the jury, and the sentence of the court, after the verdict of guilty was rendered by the jury.

A brief history of the case will be necessary in order to appreciate the nature of the testimony, on which we design to make a few comments.

The accused, John Hendrickson, Jr., a young man twenty years of age, born of respectable parents, married, at the age of eighteen, Maria Van Dusen, a young lady of seventeen, also of highly respectable connections, well educated, amiable, and intelligent. There is no evidence that they lived together unhappily; and although the District Attorney, in his opening speech, spoke of the prisoner having communicated syphilis to his wife, there is no satisfactory proof that such had been the case; but, on the contrary, it is quite evident that she had laboured under a severe form of leucorrhœa. On the night of the 7th of March, 1853, after attending church in the evening, she retired to bed with her husband at her father-in-law's, between ten and eleven o'clock, complaining of a severe pain in her head, hips, and loins, and at two o'clock was found dead by her husband, "occupying nearly the centre of the bed, lying at full length on her back, with her hands either crossed, or lying down by her side, the bedclothes covering her person." A coroner's inquest was held the same evening, and a *post-mortem* examination ordered, which was made thirty-six hours after death by Dr. J. Swinburne, Dr. Ingraham, and the coroner, Mr. Smith, being present. Four days after, the body was disinterred, and further *post-mortem* dissection made, the first, for some reason not specified, having been incomplete. The principal appearances noticed are the following: Great pallor of the surface; calm expression, and no distortion of features; sugillation on the posterior parts of the body; great rigidity and *elasticity* of the muscles; lungs and heart healthy; heart empty of blood, except a small clot in the right auricle; the vena cavas partly full of dark fluid blood; stomach and small intestines empty; liver healthy; gall-bladder half full of bile; mucous coat of stomach very red, lined with a thick reddish mucus, corrugated, the stomach itself contracted to two inches in diameter, and its coats hypertrophied; the mucous

coat of duodenum also corrugated, and more congested than that of the stomach; uterus enlarged to twice its natural size, hardened, and its cervix slightly ulcerated, with adhesions to the rectum and small intestines; ovaries considerably enlarged; spleen and pancreas healthy; urinary bladder contracted; brain healthy, no congestion; tongue white, and a little swollen; a small ecchymosed mark on inside of lower lip, showing a cut about a quarter of an inch in length (throat, œsophagus, spinal cord, and lower portion of intestinal canal not examined). At the next examination, five days after, the kidneys were found healthy, feces in the cæcum;—portions of lung, liver, and pancreas, with four ounces of blood from cavity of chest, were removed, placed in jars, and carefully covered.

To the question, on the subsequent trial, "What was the cause of death?" Dr. S. replied:—

"Acrid poison. I base it on this: I find entire emptiness of the stomach and small intestines, so far as fecal matter is concerned; also contraction and corrugation of the same to a great extent. I find in place of that a reddish *viscid* mucus adhering to the coat of the stomach and intestines; the emptied condition of the gall-bladder; the appearance of the tongue. *I inferred from these that vomiting had taken place*, and that, too, induced by some acrid matter, which would not only expel the contents of the stomach, but of the small intestines, the presence of which acrid matter would induce the vomiting."

To the question, "What would induce your belief that she vomited?" the witness replied:—

"I believe the act of vomiting is accompanied by more or less contraction of the stomach; where that act is induced by the presence of acrid matter, *the contraction will be proportional to the material used, be it more or less irritating.*" "The corrugation would be owing, in part, to the contraction of the muscles, and part to the irritating matter applied to the mucous surface." "The rigidity of the body presented the appearance of a person destroyed by anything *which would produce a sudden spasm or contraction of the muscles.* The appearance of the stomach and intestines proved to me *conclusively* that she had vomited, and to such a degree as could not be produced by ordinary causes; and I think the effort at vomiting continued until exhaustion took place. My reasons are these: *the blood, in the first place, was thrown from the centre to the surface; also, the extreme pallor of the countenance*, which always attends exhaustion from vomiting."

To the question, "Will you state, in your judgment, from what poison she came to her death?" Dr. Swinburne replied:—

"*I suppose the deceased died from aconite, from the fact of the appearance on the post-mortem examination being so identical with those of the dogs and cats,*" (experimented on by Dr. Salisbury with tincture of aconite.)

The counsel then asked: "What is the strength of your opinion, that she died of poison?" Dr. S. replied:—

"*I have no doubt of it; I have no doubt that she vomited; one of my reasons for thinking she died of poison, was her having vomited, and also the absence of congestion. Had Mrs. H. died of natural causes, more or less natural contents would have been found in the small intestines*, and they would have presented a healthy appearance, relaxed instead of contracted; also, *the circulating system would have presented a different appearance.* In all forms of death by asphyxia, syncope, apoplexy, epilepsy, and all the other forms of convulsive diseases, you have fulness or engorgement of the heart and all the important viscera." Again: "From my reading, knowledge, and experience, I am prepared to express an opinion as to what caused the morbid condition of the stomach and bowels—*it was aconite, or the principle of aconite.* Had the poisonous matter passed off with any of the fecal matter, I think the *rectum* would have pre-

sented the same appearance as the intestines, only in a less degree; can assign no possible way, except by vomiting, how the fecal matter could have been removed."

"What external appearances would you look for, where death has been caused by aconite in three or four hours?"

"*Should expect to find rigidity of all the voluntary muscles; also, extreme pallor from vomiting.*"

The testimony of Drs. Ingraham and Coroner Smith, though lacking the positiveness of that of Dr. S., just given, is of similar tenor, so far as regards the *post-mortem* appearances. Both express the opinion that death was caused "by some acrid substance introduced into the stomach." Both also were willing to swear, from the appearance of the stomach, that severe vomiting had taken place previous to death. Dr. Smith, however, was inclined to think that death was occasioned by the influence of some poison on the nervous system.

We hardly dare trust ourselves to comment on the above testimony as it deserves; for, if there is any principle established in legal medicine—one in which all pathologists agree—it is, that no positive proof of poisoning can be derived from the *post-mortem* appearances, either in the internal or external parts of the body.

"Any evidence," says Taylor, "derivable from the appearances in the body of a person poisoned, will be imperfect unless we are able to distinguish them from those analogous changes, often met with as the results of ordinary disease. These are confined to the mucous membrane of the stomach and bowels. They are redness, ulceration, softening, and perforation; all of these conditions may depend upon disease, as well as upon the action of irritant poisons."[1]

So also Guy: "The symptoms and *post-mortem* appearances produced by poisons are not peculiar to them, but may be produced by natural causes, and form a part of common diseases."[2]

And yet Dr. S., on his cross-examination, states that "he never read in any work on medical jurisprudence that a physician should not give an opinion of the death of a person from poison from the mere appearances on *post-mortem* examinations;" and thought that "a prudent person would express such opinion; and that a person could give evidence of death by *aconite* merely from the inspection of the person after death!"—also, that "inflammation never takes place except from the presence of some irritant!"

With regard to the degree of "congestion" of the mucous membrane of the stomach, &c., the sum of the medical testimony is as follows: Dr. Smith remarks, that he "did not examine the bowels or duodenum particularly, only they appeared smaller than usual." Dr. Ingraham speaks of a "reddened appearance of the folds of the stomach." If the redness of the mucous surface had presented anything very remarkable, it would probably have been brought out more prominently. Dr. Swinburne's testimony is, that "the vessels were so filled that the mucous surface looked very red, and the mucous coat was lined with a thick reddish mucus." Dr. S. regards the redness as simply "congestion, and not inflammation." There were about two ounces of reddish mucus in the stomach, which was hypertrophied, but its villous coat less congested than that of the duodenum; paler, but more corrugated than the latter. The duodenum was empty, and smaller than natural. The remainder of the intestinal canal was not examined till four days afterwards, when the body was exhumed.

[1] Taylor on Poisons, Am. ed. p. 100.
[2] Med. Jur., Am. ed. p. 544.

Admitting, then, that there was an unusual degree of redness or congestion of the mucous membrane of the stomach, it is now well established that it is not uncommon to find such appearances, not dependent on the action of poison or any assignable cause. Dr. Taylor, in his work on Poisons (p. 101, Am. ed.), remarks that—

"A person may die without suffering from any symptoms of disordered stomach; but on an inspection of the body, a general redness of the mucous membrane of this organ will be found, not distinguishable from the redness which is so commonly seen in arsenical poisoning. Several cases of this kind have occurred at Guy's Hospital; and drawings have been made of the appearance of the stomach, and are now preserved in the museum collection. A record has been kept of four of these; and it is remarkable that, although in not one of them, before death, were any symptoms observed indicative of irritation or disease of the stomach, yet in all, the stomach was found more or less reddened, and in two exclusively so. Such cases are only likely to lead into error those who trust to this *post-mortem* appearance alone as evidence of poisoning; but no medical jurist, aware of his duty, could ever be so misled."

He then gives a case of a young woman far advanced in pregnancy, who died suddenly in a fit of syncope, and where, after death, "the mucous membrane of the stomach was reddened, and thrown into rugæ." There is also an interesting case recorded in the *Annals of Hygiene*, 1835, vol. i. p. 227, where it is probable that this pseudo-morbid appearance of the mucous membrane was mistaken for the effects of irritant poison. Dr. Yellowly has also shown very clearly that the mucous membrane of the stomach often presents a high degree of vascularity and redness in cases of *sudden* death (*Med.-Chir. Trans.* 1835). He met with this appearance, as we have also in the stomachs of executed criminals, &c., and after presenting a great array of facts, adduces from them the following conclusions: 1. That vascular fulness of the lining membrane of the stomach, whether florid or dark-coloured, is not a special mark of disease, because it is not inconsistent with a previous state of perfect health. 2. That those pathologists were deceived, who supposed, from the existence of this redness in the stomach, that gastritis sometimes existed without symptoms. 3. That erroneous conclusions as to the cause of death are frequently owing to the same mistaken observations, the effects of putrefaction and spontaneous changes induced by the loss of vitality, being sometimes attributed to the action of poisons. 4. That the vascularity in question is entirely venous, the florid state of the vessels arising from the arterial character of the blood remaining in the veins for some time after its transmission from the arterial capillaries at the close of life; the appearance is, however, sometimes due to transudation only. 5. That the fact of inflammation having existed previously to death, cannot be inferred merely from the aspect of vessels in a dead part; there must at least have been symptoms during life. These positions are now also maintained by Andral and the best French pathologists, as well as those of Germany and this country; so that redness of the mucous membrane of the stomach and intestinal canal can no longer be regarded as proving the past existence of inflammation, unless there have been symptoms during life, or other marked effects of the inflammatory process in the alimentary canal, or the discovery of the poison itself. We may also, in this connection, refer to the cases recorded by the late Prof. W. E. Horner, in the first volume of this Journal, for evidence of the same fact. Prof. Horner was one of the first to prove that there may be great congestion of the mucous membrane of the stomach, abundant mucus, and great corrugation and contraction, without previous inflammation, and where death has resulted from other diseases. In one case, where death was sudden, and no

suspicions of poisoning, he found the gastric coats thick and dense, the mucous coat thrown into numerous folds, or well-marked, elevated rugæ, and almost universally of a deep arterial red. The red corpuscles were extravasated in numerous spots and blotches. Dr. Beck also remarks:—

"MM. Nigot and Trousseau, and M. Billard, have pursued the investigation of this subject to a great extent. The former have proved by experiment, that various kinds of pseudo-morbid redness may be formed, which cannot be distinguished from the parallel varieties caused by inflammation; that these appearances are produced after death, and not until three, five, or eight hours after it; that they are to be found in the most depending parts of the stomach, and turns of the intestines; and that, after they have been formed, they may be made to shift their place, and appear where the membrane was previously healthy, by simply altering the position of the gut."[1]

It is very evident then that redness, quite equal in degree to that observed in the case of Mrs. Hendrickson, is no uncommon appearance in *post-mortem* examinations, and the redness would seem to be intense in proportion as the death has been sudden and the circulation active. The redness in the present instance was no greater than is usually met with in cases where death is as sudden. The "reddish mucus" in the stomach may, perhaps, be satisfactorily accounted for from the presence of the colouring matter of tomatoes, which the deceased ate freely shortly before her death. The microscope might have settled this question definitely.

The other phenomena mentioned, on which considerable stress appears to have been laid, viz: pallor of the surface, and rigidity, and elasticity of the muscles, &c., are even less significant or characteristic of poisoning than the slight congestion already noticed. Neither can be considered as indicative of modes of death; their absence, indeed, might be worthy of note in a suspected case, but not their presence. In all cases of death from irritant poisons, especially the narcotico-irritants, which we have had an opportunity of observing, there have been crimson or livid-coloured patches on various parts of the body, and considerable tympanitis, though we cannot affirm that these phenomena are invariably present in such cases. There has also been considerable bloody, frothy mucus in the mouth, fauces, and œsophagus, especially when vomiting has been severe. No such phenomena were noticed in the present instance; only we are told that the tongue was "swollen, and very white," an appearance not particularly indicative of gastric irritation. It is also worthy of note, that the "pallor" is attributed to the "blood having been thrown from the centre to the circumference, by vomiting," which we should suppose would have the opposite effect.

"The rigidity of the body," says Dr. S., "presented the appearance of a person destroyed by anything which would produce a sudden spasm or contraction of the muscles."

Here it is assumed that the deceased died from spasm, and that the spasmodic state of the muscles continued after vitality was extinguished (thirty-six hours after death). No allusion is made to the *rigor mortis*, as a common cadaveric phenomenon; but it is claimed throughout the direct testimony, that the stiffness of the body was owing to some poison which had caused severe spasm; and because the witness had noticed the same rigidity in dogs, destroyed by aconite, he does not hesitate to express the opinion that the deceased came to her death from the same poison. Now, cadaveric rigidity takes place in all classes of animals alike; coming on as soon as muscular irritability ceases, confined to the muscular system in many cases, giving extreme rigidity to the

[1] Med. Jur. vol. ii. p. 310.

body, its degree and duration, *cæteris paribus*, being directly as the muscular development; continuing longer the later it occurs, and *vice versâ;* influenced greatly by the nature of the disease, or the cause of death; appearing more speedily, and lasting a much shorter time, when death has occurred from some chronic wasting disease, as phthisis, fever, scurvy, &c., coming on slowly and being strongly developed, and lasting often for several days, when the death has been occasioned by acute inflammation of the stomach, or by irritant poisons. We have known cadaveric rigidity continue four or five days after death from cholera. Any one conversant with these facts, would hardly dare derive any positive conclusion as to the cause of death from the presence of muscular rigidity. We dismiss it, therefore, with the "pallor" and "congestion," as wholly insufficient to justify the conclusions deduced from it.

Considerable stress is also laid by the witnesses, on the "corrugation and contraction of the stomach;" but these are so often met with in death from natural causes, that no significance can be properly attached to them. Prof. Horner has shown that *rugæ* of the stomach are quite common in *post-mortem* examinations, and that they appear in cases where no stimulants or irritants have been applied, and in stomachs perfectly healthy, and that they are more frequently met with in cases of sudden death; and yet it is assumed, in the case of Mrs. H., that these rugæ were owing to the *astringent* properties of aconite, which has heretofore been regarded as a *paralysant*, and not a stimulant.

We do not hesitate to say that aconite has no power to "shrivel vegetable membranes," as claimed upon the trial; though it will destroy vegetable life; the subsequent "shrivelling" is doubtless the result of the evaporation of water, or drying.

The diminished caliber of the stomach and intestinal canal, was in all probability the result of sudden death, leaving the organic contractility of the muscular fibre unimpaired.

Extensive experiments have been made with aconite upon the lower animals; but corrugation of the stomach has never been claimed as one of its *specific* effects. It was never observed by Orfila, or by Fleming. The presence of two ounces of viscid mucus in the stomach, is regarded by Dr. F. as positive proof that an irritant had been swallowed, and yet Dr. Horner gives cases (*loc. cit.*) where the same amount of similar mucus was found in healthy stomachs. It is not unusual to meet with considerable quantities of mucus in the stomach, after death from various diseases; nor was its colour by any means remarkable under the circumstances; and yet the witness states that he "had never seen viscid mucus in a stomach after death from natural causes," nor "clinging to the coat of the stomach." The deceased had eaten little or nothing during the day preceding her death (Dr. S. says ten or eleven hours), and speaks of the empty condition of the stomach and small intestines as a remarkable circumstance, and one which proved conclusively that severe vomiting had occurred previous to death. "I believe," says he, "that all the contents of the stomach and small intestines could be thrown up, and not a particle or trace be discovered in any part;" "the quantity in a healthy person would be from a pint to two quarts;" and "this might be thrown up in an hour." To another question he replied: "A healthy stomach might expel its contents in about three or four hours;" and "considerable portions of the fecal matter would remain in the small intestines twenty-four hours."

Criticism on such statements is a work of supererogation; according to Beaumont, the *longest* time required for digesting any substance, in the stomach of St. Martin, was five and a half hours, the average less than three. Food does

not long remain in the duodenum, and the *jejunum* is so called, because it is generally empty. It would have been strange, indeed, and contrary to the usual course of things, if anything had been found in the stomach and small intestines. Had the deceased eaten supper at six o'clock (and there is no evidence that she ate any dinner or supper, but very satisfactory proof that she did not, to any amount), it would not have been strange, death occurring eight hours after, to find the stomach and small intestines empty.

It was proved by at least two witnesses, that there were no evidences of vomiting in the room where the deceased slept, when found dead, there being but one vessel in the chamber, and that half filled with urine alone. There were also several persons sleeping in the house; but no one heard any noise from vomiting, or any other cause. The inference, then, that the empty state of the stomach, &c. (two ounces of mucus excepted), indicates previous vomiting, is just as unfounded and unsustained by facts as the former. Dr. Salisbury testifies that

"The *stomach* was much contracted, and drawn into folds and ridges; not more than half as large as the stomach in its natural state; much congested; the mucous coat covered with a whitish substance resembling viscid mucus, and this was covered with the blood." "The *duodenum* was contracted longitudinally and transversely, highly congested, the inner coat covered with viscid mucus, mixed with a slight quantity of a whitish matter, resembling chyme, and this was mingled with blood." "The jejunum was in a high state of congestion, contracted, its mucous coat covered with mucus, and a whitish substance resembling chyle, tinged with blood, the contraction less than in the duodenum."

The appearance of the *ilium*, Dr. S. states, was similar to that of the other small intestines, while the cæcum, colon, and rectum, were half filled with fecal matters, those in the rectum being "dry and hard." Dr. S. inferred that there was a "*tendency to purging*, because the fecal matter in the cæcum was thin and watery;" also, that vomiting had occurred from "the contracted condition of the stomach, its emptiness; the emptiness of the small intestines, and the high state of congestion and effusion of blood in these organs." "Had Mrs. S. taken her usual meals on *Saturday*," says Dr. S. (death occurred between one and two o'clock on Monday morning), "I should have expected to have found food in the small intestines."

Comment on this evidence is altogether unnecessary. Why the mucus in the *ilium* should be more deeply tinged with blood than that of the stomach, we are not informed. No one, on reading the evidence, can doubt that the stomach and small intestines were empty in obedience to the laws of their economy; the natural result of the performance of their healthy functions.

The gall-bladder was half full of bile, which, as we believe, is entirely inconsistent with the idea that severe vomiting had occurred. Dr. Swinburne says: "The gall-bladder does not become necessarily empty by vomiting; it has no peristaltic action."

Such violent vomiting, however, as is claimed to have occurred in this case, must, by compression, have forced all the bile from its cyst.

In short, the *post-mortem* appearances do not justify the inference that Mrs. Hendrickson vomited during the last hours of her life. This belief has been expressed by all the physicians, whose opinions have been sought for, including Prof. A. Clarke, of New York, A. March, P. Van Olinda, M. F. Cogswell, T. Hun, J. H. Armsby, J. P. Boyd, B. P. Staats, and T. R. Beck, of Albany. The New York Pathological Society, by a unanimous vote,

"*Resolved*—That the statements made by Dr. Swinburne, as printed in the *Report of the Trial of Hendrickson*, by Barnes and Havenor, Albany, 1853, con-

cerning the *post-mortem* appearances, as described by him in the case of Mrs. Hendrickson, in nowise justifies the opinion that death was produced by vomiting, or was caused by the administration of aconite; such appearances, especially those relating to the condition of the stomach, being often found in *post-mortem* examinations where no vomiting had occurred, and where no aconite had been taken before death.

"*Resolved*—That the *post-mortem* examination, as detailed by Dr. Swinburne, is faulty, wanting in detail as regards the condition of several important organs, and omitting to examine altogether the trachea and larynx, affections of which are known to produce sudden death."

The spinal cord was only examined down to the second cervical vertebra, and it is well known that death sometimes suddenly occurs from the rupture of a bloodvessel within the spinal canal.

In this connection, it will be appropriate to consider briefly the properties of aconite. Belonging to the natural family *Ranunculaceæ*, the *Aconitum Napellus* has generally been supposed to participate in the acrid properties of that class of plants, and hence it has been usually described as an *acrid narcotic*. It is now, however, well ascertained, that the aconite has very feeble, if any, acrid properties, the effects, such as nausea, and vomiting, &c., commonly produced by it, being occasioned by its violent action on the nervous system. Christison says its *acrid* powers are "doubtful or feeble." Pereira and Fleming do not admit that it possesses any. Taylor and Thomson also coincide in this opinion. Its peculiar effect upon the organs of taste, such as tingling, numbness, sense of heat, &c., Dr. Fleming shows to be a property belonging to its narcotic or sedative principle, and the measure of its activity as a poison. The aconite is not a stimulant, but a pure *paralysant*.[1]

The importance of a correct knowledge of the properties of plants, is well illustrated in the case of Mrs. Hendrickson. Had it not accidentally come to light that some druggist at Albany had sold an ounce of *tincture of aconite*, to some unknown person previous to the death of Mrs. H., and had it not been erroneously assumed that aconite was a powerful *acrid*, no one would have ever suspected it to have been the cause of death, nor would any such interpretation have been given to the *post-mortem* appearances.

It is claimed in the present case, that nearly or quite an ounce of the saturated tincture of aconite (of which ten drops have proved fatal), was administered to Mrs. H., in consequence of which she vomited an hour or more incessantly, and then died from exhaustion, with composed and placid features, &c., with the *post-mortem* appearances already noticed.

The experiments of Dr. Fleming upon the lower animals with aconite, prove conclusively that neither the plant, nor any of its preparations, produces vas-

[1] MM. Geiger and Hesser, two distinguished French chemists, have investigated the properties of aconite with much success. They speak of it as follows: "Cette substance alcaline, ne crystallise pas; elle est inodore; elle a une saveur amerè *sans âcreté;* elle n'est pas volatile; peu soluble dans l'eau tres soluble dans l'ether et surtout dans l'alcool," &c. (*Traité Thérap.* &c., par A. Trousseau and H. Pidoux, tom. ii. p. 118.)

MM. Merat and De Lens, in their *Dictionnaire Universel de Materièrre Médicale*, vol. i. p. 58, have described the properties of aconite very accurately, and in few words as follows: "Pris à la dose d'une drachme on deux il [the root], produit un véritable empoisonnement. D'abord les sujets éprouvent une ardeur brûlante, une soif ardente, des vertiges se declarent; de la cardialgie, des vomissements ont lieu, ainsi que des coliques atroces, avec déjections alvines; de la somnolence le manifeste, accompagnée des convulsions et d'une agitation extraordinaire; des sueurs froides et la mort viennent terminer cette scène de douleur au bout de deux à trois heures."

Sir B. Brodie as well as M. Orfila made many experiments with aconite on animals, and the latter describes the stomach and intestines as *free from inflammation*.—*Med. Leg.* ii. p. 54.

cularity in any membrane to which it is applied, even the lips and tongue when burning and tingling from its topical action; that this is purely a nervous phenomenon, and that inflammatory redness of the alimentary canal is never observed in animals poisoned by it.[1] The prominent symptoms, according to this careful observer, are weakness and staggering, gradually increasing paralysis of the voluntary muscles, slowly increasing insensibility of the surface, more or less blindness, great languor of the pulse, and convulsive twitches before death, with great contraction of the pupil and impairment of the muscular irritability, and, of course, loss of muscular power. When it proves fatal to the human subject, it generally does so by inducing extreme depression of the circulation, or paralyzing the muscles of respiration. Dr. Fleming states "that it may also kill, by an overwhelming depression of the nervous system, proving fatal in a few seconds, without arresting the action of the heart; and secondly by asphyxia, or arrest of respiration, the result of paralysis, gradually pervading the whole muscular system, respiratory as well as voluntary. Such are the results when very large doses are taken." The least variable symptoms in the human subject are, first, numbness, burning and tingling in the mouth, throat, and stomach; then sickness, vomiting, and pain in the epigastrium; next, general numbness, prickling and impaired sensibility of the skin, impaired or annihilated vision, deafness and vertigo; also frothing at the mouth, constriction at the throat, false sensations of weights or enlargements in various parts of the body; great muscular feebleness and tremor, loss of voice, and laborious breathing; distressing sense of sinking and impending death; a small, feeble, irregular, gradually vanishing pulse; cold clammy sweat, and pale, bloodless features, together with perfect possession of the mental faculties, and the tendency to stupor or drowsiness. Finally, sudden death at last, as from hemorrhage, and generally in a period varying from an hour and a half to eight hours. In a case observed by Fleming, where the *tincture of the root* had been taken, the symptoms began in a few minutes. But in a case recorded by Pereira (*Mat. Med.* vol. ii. p. 1806), where the root was eaten in mistake for horseradish, no effects were observed for nearly an hour. Generally, however, the tincture and the alkaloid act with very great rapidity, the effects following on absorption, which may happen in from two to eight minutes, according to the condition of the stomach and the general system at the time.

It must be recollected, however, that the effects of aconite on the system are not uniformly the same. Some anomalies have from time to time been observed; such as convulsions and slight spasmodic twitches of the muscles, owing doubtless, to venous congestion, the result of partial asphyxia; stupor and insensibility, due to the same cause, though they may have been confounded with extreme nervous depression and faintness; delirium; congestion of the mucous membrane of the stomach, &c. This, however, we have seen, is generally found in cases of sudden death. Nausea, vomiting, and pain in the epigastrium, are not regarded by Dr. Fleming as evidence of gastric irritation, as they may all be owing to the same local nervous impression which is produced on the organs of taste. He denies that *purging* is ever produced by aconite. Pallas, however, mentions that three out of five persons who took *tincture of aconite* died in two hours, with burning in the throat, vomiting, colic, swelling of the belly, and purging. (*Thèse Inaugurale*, Paris, 1822.) Degland relates an instance where four persons took the tincture by mistake, and

[1] Prof. A. T. Thomson remarks (*London Dispensatory*): "Although aconite operates topically, yet dissections of fatal cases have not displayed any particular marks of inflammatory action." (P. 181.)

three of them were seized with burning pain from the throat to the stomach, sense of swelling of the tongue and face, colic, tenderness of bowels, vomiting, and purging; one died in two hours, and one in two and a half hours.

The authenticity of these cases, of Pallas and Degland, is, however, doubted both by Fleming and Pereira, who suppose that it was the tincture of some other root that was taken. In the cases recorded by Pereira, no purging occurred. Diarrhœa, if it does occur in aconite poisoning, is an extremely rare phenomenon.

Morbid Appearances.—Ballardini, who met with twelve fatal cases of poisoning with aconite,[1] represents the pia mater and arachnoid as much injected; much serosity under the arachnoid and at the base of the brain; lungs considerably engorged with blood; heart, and great vessels contained a little black fluid blood; villous coat of stomach spotted with red points; the small intestines presenting red patches, and much mucus. In a case mentioned by Pereira, there was venous congestion of the head and chest, and great engorgement of the lungs and right side of the heart.[2] It cannot be denied that great redness of the mucous membrane of the stomach and small intestines, has been occasionally observed in cases of poisoning by aconite—as in the cases which occurred some years since at Lille, in France, and recorded in the *Edinburgh Medical and Surgical Journal*, vol. xxviii. p. 452. In some cases, the mucous membrane of the stomach has been found of a light reddish-brown colour; and in others still, quite natural in appearance. There is no instance on record, we believe, where aconite has produced inflammation of any of the mucous membranes or organs of the body, but merely congestion; and this may be of the brain, lungs, liver, spleen, and mucous surfaces; no softening or effusion of lymph, or other mark of inflammation, has ever been found in connection with the patches of redness produced by aconite. Its immediate action in lowering the action of the heart, and arresting or retarding the respiratory movements, proves conclusively its purely sedative and paralyzing power. The appearances on dissection in Mrs. H.'s case do not correspond with those generally observed in cases of poisoning by aconite.

Chemical Testimony.—Chemical evidence is very justly regarded as the most decisive of all the kinds of proof in medico-legal inquiries. In the present instance, however, the anatomical evidence appears to have been regarded as very important.[3] The case, however, undoubtedly turned chiefly upon the chemical evidence. This was deemed conclusive. It may have cost John Hendrickson, Jr. his life. Alas! as Raspail has observed, "it is never too late to unlearn an error, but an incorrect testimony, once given in a court of justice, can never be recalled. The sword of the law does not retrace its steps, as the opinion of an experimental chemist may."[4]

Dr. James H. Salisbury, chemist, having charge of the New York State Laboratory, testified as follows:—

"With the portions of the subject submitted to me, I proceeded to make my tests for poisons. In this case, my chemicals were all pure, my implements

[1] Annali Universali di Medicina, 1840, iii. 635.

[2] Christison on Poisons, Am. ed. p. 668, 669, &c.

[3] Judge Marvin, in his charge to the jury, says: "We learn from the evidence of Dr. Swinburne, and other medical men, that *post-mortem* examinations will generally disclose the cause of death!" After minutely describing the morbid appearances, because, he says, "they form the facts or basis on which the professional witnesses found their opinion," yet he states that the coroner, Dr. Smith, could not find (though present at the first *post mortem*) the marks of inflammation or congestion in the stomach described by Dr. Swinburne!

[4] Organic Chemistry, p. 525.

and vessels clean. First, I took a small portion of the stomach, its mucous surface, and a small portion of the duodenum; tested first for prussic acid—did not detect its presence; then tested for some of the mineral poisons, first for arsenic, then for corrosive sublimate, the antimonial compounds, the mineral acids—such as muriatic, nitric, and sulphuric acids—also tested for oxalic acid; did not detect the presence of any of these. Next tested for morphine, strychnine, stramonine—also for other poisons, none of which I discovered. I then tested for *aconite;* the tests indicated *aconitine*, the poisonous principle of aconite. Took a small portion of the stomach and duodenum, digested it in alcohol over a water-bath, then filtered; evaporated the filtrate partially; the oily matter rose to the surface; this I separated by decantation, and then absorbed it from the surface by bibulous paper; then mixed the solution with *purified animal charcoal*, agitating it for some little time after mixing; filtered; and to this solution I applied my tests, as follows: I boiled a small portion of this solution with sulphuric acid; the solution was turned a deep port-wine red colour. I then boiled a small portion of the solution with hydrochloric acid; this turned the solution to light port-wine red colour. Then boiled a small quantity of the solution with nitric acid; the solution remained clear, with no change of colour. *From these tests I inferred the presence of aconitine.* I repeated these tests several times with the same results. The stomach and duodenum were what I made my preliminary tests with; these tests are what are laid down for *aconitine* by the best authors. I had made these tests previously, and had also made aconitine my special study. My tests produced the same results. I have applied them since with similar results. For two years previous to this, I had paid much attention to vegetable alkaloids, and among them especially aconitine, the poisonous principle of aconite. On the 15th of March I commenced the process of *analysis* for the purpose of separating *aconitine*, if present in sufficient quantity. I divided a portion of the remaining portions of the stomach and duodenum, and their contents, the small intestines, a portion of the liver, and a portion of the blood, into two equal parts. One of these portions I digested in alcohol for several hours over a water-bath; then filtered; partially evaporated; separated the oily matter by decantation and absorption; evaporated nearly to dryness; mixed with the alcoholic extracts pure caustic potassa; distilled; heated the distilled matter with dilute sulphuric acid, sufficient to neutralize it; evaporated this over a water-bath; treated it with pure alcohol, between 74 and 80 per cent.; filtered it; evaporated it nearly to dryness; treated the residue with pure caustic potassa, and again distilled; evaporated this slightly, and set it aside for future use. The other and second portion was digested in alcohol over a water-bath for several hours; filtered; evaporated partially; separated oily matter by decantation and absorption; evaporated nearly to dryness; treated the alcoholic extract with dilute sulphuric acid and distilled water; filtered; then evaporated it partially; treated the solution with ammonia to a slight excess; a precipitate was formed; this was carefully washed by a small quantity of water; this precipitate was redissolved in dilute sulphuric acid and distilled water; added to this solution a small quantity of purified animal charcoal, agitating for some minutes, and then filtered it; evaporated the filtrate slightly at a low temperature; added ammonia in slight excess; a precipitate was formed; this I carefully washed with a small quantity of distilled water; this result I mixed with the result obtained by the other process; in all there was about two-thirds of a teaspoonful. I was from the 15th to the 19th of March in getting through this process; it was going on day and night. In testing for this matter, I placed it on my tongue; it had a bitter taste; a sparkling (?) sensation at first, which, in three or five minutes, turned into a numbness, producing a stiffness of the surface; the sensation produced was very much like that in the foot when it is said to be asleep. This matter, which I separated by the process just mentioned, I gave to a cat; gave it in small pieces of beefsteak; in about half an hour she exhibited a choking sensation and swallowing; this was followed by slight contraction of the muscles, twitchings, which moved the limbs slightly, and this by a tendency to vomit. These spasms lasted from one to two minutes; considerable stupor succeeded; she lay down upon her

side and breathed heavily, as though she was under the influence of some narcotic; this lasted for some time; it gradually passed off, and in about three hours she was quite natural again. On the 29th of March, I gave this cat six drops *tinct. of aconite.* In administering it, I opened her mouth, held her head back, and poured the tincture immediately down her throat; after five or ten minutes she commenced swallowing; in fifteen minutes she commenced vomiting slightly; this vomiting continued for twenty-five minutes, when she became very weak, and fell upon her side; the vomiting here ceased; she breathed heavy and slow, and in one hour and a half after the poison was taken she died. The *post-mortem* was made seven hours and a half after she died. The stomach and intestines were found very much contracted; about one-third the usual size. The intestines were very much contracted and rigid: the walls thick. We then opened the stomach, and found a very high state of congestion; it was very much contracted on itself, and thrown into folds. The mucous coat was covered with a small quantity of mucus, tinged with blood. The duodenum was contracted; very much congested. The mucous coat was covered with mucus, tinged with blood. There was one or two places in the lower portion of the duodenum, where the mucus was tinged with bile; the whole was tinged with blood. The jejunum was considerably congested and contracted. The mucous coat was covered with mucus, tinged with blood. The ilium had much the same appearance as the jejunum, except that the mucus which covered its surface was of a slightly different colour—pure white; there was a little bile here. I found no fecal matter thus far; in the cæcum I found fecal matter; it was partially digested food, mixed with white frothy matter; the upper portion of the colon also contained fecal matter, which was thin and watery; not as thin and watery as that in the cæcum. The fecal matter grew harder as it approached the rectum; there was no purging. The urinary bladder was very much contracted; there was no water in it; the gall-bladder was about half full."

Question. "After your researches and anatomical experiences, are you ready to swear that Mrs. Hendrickson was poisoned by aconite?"

Answer. "In my opinion she was poisoned by aconite."

Dr. S. further testified that—

"The *post-mortem* appearances of the stomach and intestines, and those of the animals killed with aconite, are not produced by any other substance known to him; and that he knows of no disease which will produce such appearances."

In examining the chemical evidence, we must first express our regret that the details of the processes employed by Dr. Salisbury, are so imperfectly given. We can only judge of the correctness of the results from the very meagre account presented in the evidence as published.[1] We shall, however, if we mistake not, discover such errors in the proceedings as to vitiate in a great degree, if not wholly, the conclusions which are drawn from them. We are first given to understand, that with "a small portion of the mucous surface of the stomach and duodenum," Dr. S. first tested for *prussic acid*, then for *some* of the *mineral poisons* (arsenic, corrosive sublimate, the antimonial compounds, the mineral acids, muriatic, nitric, and sulphuric), then oxalic acid; also for *morphine, strychnine, stramonine,* (daturia?) and also "for other poisons," but without success. These various processes, we presume, must have taken a considerable portion of the "small portion of mucous membrane" employed. We have no means of judging of the accuracy of the tests employed, for few, if any, details whatever are given.

Dr. S. then states "that he *tested* for *aconite,* and found it by proceeding as

[1] We assume, however, that the evidence of Dr. S. is correctly printed, as it was done at the place of his residence, and contains notes by himself, added after his testimony was given.

follows: He took, as before, a small portion of stomach and duodenum, digested it in alcohol over a water-bath, then filtered, evaporated the filtrate partially; the oily matter rose to the surface; this he separated by decantation, and then absorbed it from the surface by bibulous paper; then mixed the solution with purified animal charcoal, agitating for some time after mixing, filtered, and applied his acid tests as follows: First, he boiled a small portion of the solution with sulphuric acid; the solution was turned a *deep port-wine red colour;* then the same process with hydrochloric acid gave a *light port-wine red colour;* then with nitric acid, and the *solution remained clear;* and from these tests, Dr. S. says, "*he inferred the presence of aconitine,*" but on what grounds we are totally at a loss to discover. *Sulphuric acid* boiled with aconitine gives *a dark brown tint,* according to Taylor, instead of a *deep port-wine red colour;* and animal matters, boiled with sulphuric acid, will give the latter colour *without the presence of aconitine;* so say Taylor and the best authorities on legal medicines. (*Taylor on Poisons,* Am. Ed. p. 615.)

Prof. Emmons, in a published letter to Gov. Seymour, of New York, states as the result of his experiments, that "sulphuric acid boiled with the tincture of aconite, obtained from the same sample as that supposed to have been sold to Hendrickson, *lost most of its red colour,* and became quite pale; boiled with pure aconite, the solution remained colourless, and the same result took place when boiled with nitric acid;" but that when he added oil or animal matters to the mixture of aconite, then he "obtained the red colours spoken of by Dr. Salisbury, and the same also occurred *when he employed the same tests on these animal matters alone.*"

We think there can, therefore, be no doubt whatever that the results arrived at by Dr. S. were entirely due to the presence of organic matters, and not to aconitine, although the colour obtained by Dr. Emmons with sulphuric acid differs from that laid down by Taylor as resulting from a solution of pure aconitine.

But we are not called upon to reconcile this discrepancy; our aim is merely to show the incorrectness of the inference that aconitine was discovered by the process employed by Dr. S.[1]

To confirm the conclusion arrived at by these tests, Dr. S. then proceeded to separate a sufficient quantity of aconitine for experimenting with it on animals; and he "divided a portion of the remaining portions of the stomach and duodenum and *their contents,* the small intestines, a portion of the liver, and a portion of the blood, into two equal parts;" one of these he digested in alcohol over a water-bath, for several hours, "then filtered, partially evaporated, separated the oily matter by decantation and absorption, evaporated nearly to dryness, mixed pure caustic potassa with the alcoholic extracts, distilled, treated the distilled matter with dilute sulphuric acid sufficient to neutralize it; evaporated this over a water-bath, treated it with pure alcohol; filtered and evaporated it nearly to dryness; treated the residue with pure caustic potassa, and again distilled; evaporated slightly and set aside for future use."

The above process is that which was sometimes formerly pursued by chemists when examining *for a liquid and volatile alkaloid,* but why it should have been resorted to in the present instance, we are at a loss to understand; as Dr. S. states, in his cross-examination, that he believes aconitine "is a *fixed body.*" We are still more at a loss to know why Dr. S. did not test the matters obtained, in order to ascertain whether he had discovered aconitine or any

[1] Before commencing the analysis, it would have been as well, perhaps, to have tested the substances employed, and seen whether they were *acid* or *alkaline.*

other alkaloid by this process. He neither tasted of it, examined it with a microscope, or gave any of it to the lower animals.

But the course pursued by Dr. S., even on the supposition that a volatile alkaloid was present, is not one which will approve itself to the minds of scientific chemists, as the most eligible.

In the first place, in examining the tissues of organs for a vegetable alkaloid, the organ should be divided into very small portions, then the mass moistened with pure alcohol, and expressed strongly; and so, by further treatment, exhaust with alcohol the tissues of everything soluble; then the liquid so obtained should be treated in the same way as a mixture of suspected matter and alcohol. Moreover, it is a principle now well established in medico-legal researches, that we should never use *animal charcoal* for decolorizing liquids, while searching for the alkaloids; for the very good reason that we, by so doing, may lose all the alkaloid in the suspected matter, animal charcoal having the power, as proved by M. Stas, of absorbing these substances, while, at the same time, it fixes the colouring and odoriferous matters. This error, then, would vitiate the results obtained by Dr. Salisbury, even were his other processes unexceptionable.

The true and only correct mode of proceeding, whether the alkaloid be fixed or volatile, would be that pointed out by M. Stas, who adopted it with such brilliant success in the case of M. Fouguies, who was poisoned by his brother-in-law, Count Bocarmé, with *nicotine*. (See this *Journal* for Jan. 1854, p. 263, *et seq.*)

To the matter obtained by digesting the different organs suspected of containing the alkaloid with strong alcohol, twice their weight of pure alcohol should be added, and afterwards, according to the quantity and nature of the suspected matter, from ten to thirty grains of *tartaric* or *oxalic* acid should be added (the *tartaric* is preferable), we are then to introduce the mixture into a flask, and heat it to 160° or 170°. After it has cooled, it is to be filtered, the insoluble residue washed with strong alcohol, and the filtered liquor *evaporated in vacuo;* or, if the operator has not an air-pump, the liquid should be exposed to a strong current of air, at a temperature of not more than 90° F. If the residue, after the volatilization of the alcohol, contains fatty or other insoluble matters, the liquid should be filtered a second time, and then the filtrate and washings of the filter evaporated in the air-pump till nearly dry; or, if no air-pump is at hand, it should be placed over a bell-jar, over a vessel containing concentrated sulphuric acid. The residue is then to be treated with cold anhydrous alcohol, taking care to exhaust the substance thoroughly; and the alcohol should be evaporated in the open air, at the ordinary temperature, or what would be still better, *in vacuo*. The acid residue should then be dissolved in the smallest possible quantity of water, and the solution introduced into a small test-tube, and a little pure powdered bicarbonate of soda, or potash added, little by little, till a fresh quantity produces no further effervescence of carbonic acid. The whole should then be agitated with four or five times its bulk of pure ether, and left to settle, when the ether swimming on the top is perfectly clear, some of it should be decanted into a capsule, and left in a very dry place to spontaneous evaporation. We are now prepared to proceed to examine for a volatile or a fixed alkaloid, as the case may be; and no process less precise or carefully conducted, should be deemed worthy of confidence, in a case where life or death is hanging on the result.

Now if a *liquid alkaloid* be present, if we evaporate the ether, we shall have remaining on the inside of the capsule some small liquid striæ, which fall to the bottom of the vessel; and by the heat of the hand alone, the con-

tents of the capsule will expel an odour more or less disagreeable, according to the nature of the alkaloid (if it possesses odour); it may be pungent, suffocating, irritant, or simply disagreeably narcotic, modified by an animal odour. If any traces of a volatile alkaloid be discovered, then we should add to the contents of the vessel, from which we have decanted a small quantity of ether, one or two fluidrachms of water, acidulated with a fifth part of its weight of pure sulphuric acid, then agitate for some time, leave it to settle; pour off the ether swimming on the top, and wash the acid liquid at the bottom with a quantity of ether, as most of the sulphates of the alkaloids are insoluble in ether, and the others but partially so. The water acidulated with sulphuric acid will contain the whole, or a greater portion of the alkaloid in the solution, while the ether will retain all animal matters which it has taken from the alkaline solutions. To extract the alkaloid from the solution of the acid sulphate, an aqueous and concentrated solution of potash or caustic soda is to be added; the mixture is agitated and exhausted with pure ether; the ether dissolves the ammonia, and the alkaloid is now free. The ethereal solution is exposed, *at the lowest possible temperature*, to spontaneous evaporation; nearly all the ammonia volatilizes with the ether, and the alkaloids remain as residue; and in order to separate every particle of ammonia, the vessel containing the alkaloid is to be placed, for a few minutes, in a vacuum over sulphuric acid, and the organic alkaloid is obtained with all the physical and chemical characters belonging to it.

By pursuing the process above detailed, M. Stas succeeded in detecting *nicotine* in the blood from the heart of a dog, poisoned by a very minute quantity of this substance, introduced into the œsophagus. He also positively determined its existence in the blood generally, by the tests of *odour*, *taste*, and *alkalinity*; also the chloroplatinate of the base *perfectly crystallized in quadrilateral, rhomboidal prisms.*

We believe if a volatile alkaloid had been present in the matters operated on by Dr. Salisbury, he could not have succeeded in detecting it by the processes which he employed.

Nor was the course pursued in relation to the second portion, which was examined for a *fixed alkaloid*, less liable to criticism. This was digested in alcohol over a water-bath, for several hours, then filtered and partially evaporated; the oily matter separated by decantation and absorption; evaporated nearly to dryness; the alcoholic extract treated with pure distilled water and filtered; the filtrate evaporated nearly to dryness; the water extract treated with dilute sulphuric acid and distilled water filtered; then evaporated partially, and the solution treated with ammonia to a slight excess; the precipitate formed was carefully washed with a small quantity of water, and redissolved in dilute sulphuric acid and distilled water. To this solution a *small quantity of purified animal charcoal* was added, then agitated for some minutes and filtered; the filtrate evaporated slightly at a low temperature, and ammonia added in slight excess. The precipitate formed was washed with a small quantity of distilled water, and the result was mixed with that obtained by the former process; "and in all there was about two-thirds of a teaspoonful," it requiring four days and nights to complete the process.

The same fatal error was here committed as before, by using animal charcoal, supposing the alkaloid to be a fixed body. Going back to our first process, it may happen that the evaporation of the solution resulting from the treatment of the acid matter, to which bicarbonate of soda has been added, may leave or not a residue containing an alkaloid. If it does, then a solution of caustic potash or soda is to be added to the liquid, and it is to be agitated

briskly with ether. This dissolves the vegetable alkaloid, now free and remaining in the solution. In either case, we exhaust the matter with ether; and whatever be the agent that has set the alkaloid free, whether bicarbonate of soda, or potash, or caustic soda, or potash, it remains by the evaporation of the ether on the side of the capsule, as a solid body, or more commonly as a colourless, milky liquid, holding solid matters in suspension, and having the physical, chemical, and toxicological properties of that peculiar alkaloid.

After an alkaloid has been discovered, the next thing which scientific accuracy demands is, that it should be obtained in a crystalline state, so as to determine its form; and this is generally done by putting some drops of alcohol in the capsule with the alkaloid, and leave the solution to spontaneous evaporation. It may, however, be too impure, by contamination with foreign matters, to crystallize, when some drops of water, feebly acidulated with sulphuric acid, should be poured into the capsule, and then moved over its surface, so as to bring it in contact with the matter in the capsule. The matter contained in it will separate into two parts, one formed of greasy matter, which remains adherent to the sides, the other alkaline, which dissolves and forms an acid sulphate. The acid liquid is then cautiously decanted, which ought to be limpid and colourless if the process has been well executed; the capsule is then to be well washed with some drops of acidulated water, added to the first liquid, and the whole is evaporated to three-fourths, *in vacuo*, or under a bell-jar, over sulphuric acid; a concentrated solution of pure carbonate of potash is then added to the residue, and the whole liquid treated with absolute alcohol. This dissolves the alkaloid, while it leaves untouched the sulphate of potash, and excess of carbonate of potash. The evaporation of the alcoholic solution gives the alkaloid in crystals, if crystallizable. With regard to aconitine, it is apparently crystalline, the fragments, according to Pereira, appearing under the microscope like thin plates of chlorate of potash, varying greatly in shape, though the triangular form is most common. Dr. Pereira states that he could discover no distinct crystals. But Dr. Salisbury neither gives us the physical nor chemical properties of what he supposed to be aconite.

We have given the improved process of M. Stas of searching for the vegetable alkaloids, because of its superiority to former methods;[1] and it is now acknowledged by the ablest chemists as the most accurate of any yet known. We have seen that the evidence of the existence of aconitine, from the mode of proceeding, and the tests employed by Dr. S., is inconclusive, and comparatively worthless; and it is no less evident that that furnished by his subsequent analysis is equally valueless. It is so, because there is observable, through the whole processes, a departure from those rules which apply to such cases, and because the most common precautions for securing accuracy were neglected. The results, colourations, which were supposed to indicate the presence of aconitine, have been demonstrated to be owing to the action of the acids employed on organic matters. Reasoning by exclusion, which should be rigidly carried out in such cases, and to its utmost limits, would have led, and without much labour, to the correction of this first fatal error. If the substance found was aconitine, as believed, then, as no fact is better known than that this is a *fixed body*, why the very unscientific process of separating it by distillation, as a volatile substance? Dr. S. states, in his testimony, that he had made *aconite* a subject of particular study for two years previously, and yet he had not learned whether it was or not a fixed

[1] Orfila on Nicotine. Paris, 1851.

alkaloid; and that "he was not sure by which of his processes aconitine was obtained, as he tasted of neither before they were mixed." He might have been very sure he had not obtained it by distillation, for it was a chemical impossibility; he might have been pretty confident, however, that *ammonia* could thus be procured. We shall not, however, enter into a detailed criticism of the second analysis, because we conceive it to be unnecessary. With regard to the character of the precipitate formed, it is most probable that it was *phosphate* and *lactate of lime*, derived from the animal fluids employed in the analysis; but, as the physical and chemical characters of this precipitate are nowhere described, we are wholly left to conjecture. Here, at this point, by simple chemical methods, proof could have been accumulated which would have put the matter forever at rest; as it is, no one is satisfied, and every one is incredulous. All the first chemists of the country, including the Sillimans, Dana, Torrey, Chilton, Wells, Ellet, Bacon, Hayes, Porter, Kent, Emmons, Beck, &c., have published to the world their opinion, that, in their judgment, "no chemical result stated by Dr. Salisbury furnishes satisfactory evidence of the existence of aconitine or its compounds, in the fluids or organs submitted to examination."[1]

If the reader will compare the process of M. Stas, as we have detailed it, with that actually pursued by Dr. S., the inaccuracy and unscientific nature of the latter will be obvious. In his cross-examination, Dr. S. states that he believes that he obtained from $\frac{1}{2}\frac{1}{0}$ to $\frac{1}{2}\frac{1}{5}$ gr. of aconitine in the matters analyzed. Prof. Emmons states that there does not exceed $\frac{1}{3}\frac{1}{8}$ gr. in ℥j of Burrough's tincture; and Dr. Reid stated, in his testimony, that he had found, by actual experiment, that there is $\frac{1}{6}\frac{1}{4}$ gr. in ℥j of the tincture. (It is only claimed that ℥j was given to Mrs. Hendrickson!)

Dr. S. states that he tested the matter which he had obtained (the precipitate) by placing it on his tongue; that "it had a bitter taste, a sparkling (?) sensation at first, which turned into a numbness in a few minutes, producing a stiffness of the surface," &c. The substance, being the precipitate ($\frac{1}{2}\frac{1}{5}$ gr.), to which a small quantity of water had been added, amounted, we are told, to "about two-thirds of a teaspoonful," probably. But a few drops of this was used for testing; if two drops, then but about $\frac{1}{1}\frac{1}{0}\frac{1}{0}$ of a grain was employed, to which these effects must have been owing; and this upon the supposition that the aconitine was equally distributed throughout the mixture. The whole remaining portion was administered to a *cat*, without any one present to notice the effects but himself, and the life of an individual hanging on the result![2] The substance was given in small pieces of beefsteak. "In half an hour she exhibited a *choking sensation* and swallowing, followed by slight contraction of the muscles; twitchings, which moved the limbs slightly, and then a *tendency* to vomit; considerable stupor succeeded; she lay down on her side and breathed heavily, as if under the influence of a narcotic; this lasted some time; it gradually passed off, and in three hours she was well again."

This same cat was killed in one hour and a half, a week afterwards, by giving it *six drops* of *tinct. of aconite*. Symptoms: "swallowing, slight

[1] Alb. Eve. Journ. April 29, 1854.

[2] In the case of Count Bocarmé, M. Stas experimented with *nicotine* obtained, on small birds, as sparrows, which are extremely sensitive to the action of the vegetable alkaloids; and the *remainder* was carefully sealed and labelled, to exhibit at the trial, and for experiments before the jury. Such a course, we believe, is not unfrequently pursued on the continent, especially in Germany and France, and it is one which must commend itself to every lover of science and humanity.

vomiting, weakness, and heavy, slow breathing." *Post-mortem* appearances identically the same as in the case of Mrs. Hendrickson, or, as Dr. S. testified, "they were so similar, that it was almost impossible to distinguish a shade of difference between them." (P. 53.)

It certainly seems a strange anomaly, that while $\frac{1}{25}$ of a grain of aconitine did not injure the cat materially, *six* drops of the tincture should have proved fatal!—the whole ounce, according to Prof. Emmons, only containing but the $\frac{1}{1000}$ part of a grain, which would give about $\frac{1}{3000}$ of a grain in six drops. We do not vouch for the accuracy of this calculation; we only record it as one of the difficulties in the case.[1] Well did Mr. Wheaton, in his able defence, remark that "the cat should have died (by the first experiment) out of deference to the Doctor's opinion; or, the Doctor should have given up his opinion out of deference to the life of the cat."

We have not designed to offer any comments upon the *moral* evidence submitted in this case—any further, at least, than it is connected with the medical and legal evidence. We may, however, be allowed to say, that it lends little or no confirmation to the belief that poison was administered to Mrs. H. It was not proved that the prisoner ever purchased aconite, knew of its properties, or had it in his possession. The charges brought against his moral character, in the opening speech of the prosecuting attorney, particularly in regard to his having had gonorrhœa, and having imparted the same to his wife, were wholly unsustained by any evidence offered on the trial. The object aimed at, viz: prejudicing the minds of the jury against the prisoner, was, however, perhaps as fully attained as if the charges had been proved. There was no evidence that any vomiting had taken place; indeed, everything went to show that it had not; the countenance of the

[1] It is true, we have no precise information as to the quanti y of *aconitine* contained in the root of the plant. Prof. L. Reid, of N. Y., testified that he had obtained but one grain from one pound of the root, and believes that no more than that amount can be obtained. He also states that the taste of aconitine can be detected in $\frac{1}{1000}$ part of a grain. Prof. Emmons expressed the opinion that ℥j of the *tincture of aconite* contains but $\frac{1}{700}$ of a grain, allowing ℥ij of the root to ℥xvi of alcohol. Dr. Burroughs, in a letter recently received from him, states that he followed the U. S. P. of 1850 in preparing his tincture, which directs one pound of the root to two pints of alcohol, which would give four times the strength as estimated by Prof. E. But even this, on Prof. Reid's estimate, would give less than $\frac{1}{700}$ of a grain to ℥j of the tincture; but, as half a drachm of this tincture would be a hazardous and perhaps a fatal dose, and as this would be but the twenty-fourth part of an ounce, or the $\frac{1}{4800}$ part of a grain, we must acknowledge that there are some difficulties about these calculations which require to be cleared up. Dr. Pereira states that the one-fiftieth part of a grain of aconitine has endangered the life of an individual, and that it is by far the most poisonous principle known. Prof. Christison found 30 grains of an alcoholic extract kill a rabbit in two hours and a quarter; and this was the whole produce of three-quarters of an ounce of the fresh leaves. And, in another experiment, one-tenth of a grain introduced into the cellular tissue of a rabbit, killed it in twelve minutes. Orfila gave five drachms to a dog, and it killed him in twenty-one minutes. But the results were very various in his experiments, which he accounts for from the different pharmaceutical processes employed in making the preparations. In one instance, *e. g.*, he gave ℥ss *extract of aconite* to a dog without any effect; in another case, one-fourth of an ounce of the extract killed a dog in two hours. All experimenters agree in the opinion that it is not *narcotic*, and Pereira found that it never produced stupor, or affected the mental faculties.

With regard to *animal charcoal* absorbing the active principle of aconite along with its colouring matter, Prof. Emmons has stated that he "had employed the *tincture of aconite* as the substance operated upon, and had not only insulated the active principle in the charcoal itself, but had subsequently dissolved it out by means of alcohol." —*Letter to Gov. H. Seymour of New York, in Alb. Eve. Journ.* April 29, 1854.

deceased was mild and placid; no distortion, no dishevelling of the hair, no wrinkle or spots on her clothes, no signs of violence upon the exterior of the body, no marks of suffocation; the deceased lay as if asleep, and everything in the room in the same condition as when she retired to rest. This does not look like severe vomiting, or death from violence—unless, possibly, from chloroform—but this is not charged. The Judge (Marvin), in his charge to the jury, seems to take the ground that, if the medical witnesses, called for the defence, could not prove what was the actual cause of death, it was right to assume that the deceased came to her death by violence on the part of her husband! "It would be more satisfactory," he says, "if these medical witnesses had assigned some natural cause for her death;" as if the burden of proof rested on the prisoner, instead of the people, although he expressly disclaims any such ground. We confess that the case is enveloped in considerable mystery, but no more than enshrouds hundreds of cases of sudden death. We were called once to see a lady who, in the enjoyment of a comfortable state of health, while attending a social gathering at her own house, was seized with a fainting fit, and died in spite of all the means employed before our arrival. We made a careful *post mortem* the next day of all the vital organs, but there was no cause of death discoverable. The walls of the heart were thinner than usual, and syncope was occasioned by violent mental emotion, which resulted fatally. We have known several cases of sudden death from syncope, without any *post-mortem* lesion sufficient to account for the death. Fatal cardiac asthenia may be produced by mental emotions, and no *post-mortem* change or lesion discovered. It may have been so in the present instance. There was considerable *uterine* disease, as ulceration and congestion of the os uteri, and leucorrhœa; sympathetic disturbance of the heart may have been occasioned by mental emotion acting upon this predisposition, and death occurred as in the case already alluded to. Or, she may have inhaled chloroform to relieve pain, of which she had complained much during the day; or, she may have taken an overdose of *homœopathic* pills of aconite, of which she had a considerable quantity on hand just before her death, and which she carried in her pocket (of which none were found afterwards.)[1] Many causes might be assigned to account for her death, any one of which would be more probable than that she was poisoned by an ounce of tincture of aconite. There is one fact stated by the Judge in his charge, as of very great importance, and one which we have not as yet particularly noticed, and that is, a small *ecchymosis* or bruise on the inside of the lip. The Judge says this "furnishes overwhelming evidence of guilt to his mind." Dr. Swinburne describes it in his testimony as "a black and blue bruise between the size of a sixpence and a ten-cent piece, inside of the lower lip, and a little to one side; in it there was a cut about a quarter of an inch long; it must have occurred before death." This slight mark is exaggerated into a degree of importance it does not deserve. It was proved by the mother that the deceased complained two days before her death of a sore on the same lip. The evidence that the ecchymosis was caused during life is by no means satisfactory; indeed, no evidence is gone into, or facts detailed, which have any bearing on this point. It is stated by the mother, that violent attempts were made by her to open the mouth, for the purpose of administering a little camphor and water, as soon as it was discovered that her daughter was dead; and probably this was not long after she ceased to breathe. The ecchymosis may

[1] It is also held by some pathologists that death may suddenly occur from apoplectic congestion of the brain, and cerebral fulness disappear after death.

have been thus produced, though we think it more probable that it existed before death. Such marks, moreover, are not unfrequently caused by the teeth during a convulsive paroxysm. Standing alone, it by no means proves any violence on the part of the accused.

Another feature in this trial is worthy of mention, perhaps, and that is the great importance attached to the confident and positive statements of Drs. Swinburne and Salisbury, both young men, and comparatively inexperienced, and the little weight which seems to have been allowed to the more careful and judicious testimony of Drs. Emmons and Staats, men of age, professional skill, and enlarged experience. The *positive* statements of the former would probably, with such a court and jury, have outweighed the negative testimony of all the first pathologists and chemists of the age. The result shows very clearly the importance of qualifying and guarding our statements and opinions where facts will allow of another interpretation, and where they fall short of actual demonstration.

We have thus successively passed in review the most important features in this interesting trial, and made such comments as seemed to us appropriate to the occasion. We know none of the parties, nor are we influenced by any personal considerations whatever. Medico-legal science demands thus much, at least, at our hands. If we have spoken with undue severity, in regard to any testimony offered, it will ever be to us a source of regret. But the interests of science and humanity require that the medical and chemical evidence, on the strength of which the life of a human being has been taken, should be closely scrutinized, and in all the lights which observation, reason, common sense, and true science may furnish us. The case will stand as a precedent in medico-legal questions that may hereafter come before our courts. It is all-important to know whether the questions it involves have been decided correctly or not; whether it may be referred to as a safe or an unsafe precedent for witnesses, courts, and juries in future times. We have sufficiently indicated our opinion. We are ready, however, to retract whenever our reason is convinced that we have been led into error—not before. It is in vain, for it is too late, to lament the probable sacrifice of an innocent man; but, if what we have written serves but to throw an additional safeguard hereafter around the wrongfully accused, our purpose will have been fully accomplished. We will venture one remark more. It seems to us no less strange than lamentable, that in a case which appears to have rested solely on anatomical, or pathological and chemical evidence, the counter opinions and statements of the leading men in these departments of science in the country should have had so little weight with the executive or judicial power as not to lead to a commutation of punishment, nor even to an order for a new trial, which was demanded. We pretend not to fathom the hearts or the motives of men, but we think we see in the present instance an example and illustration of the force of outward pressure—of popular excitement and prejudice—on witnesses, counsel, judges, and jury, which makes us question, at times, whether the boasted right and privilege of trial by jury, be, indeed, a blessing or a curse.

C. A. L.

BIBLIOGRAPHICAL NOTICES.

ART. XV.—*Transactions of the Medical Association of the State of Alabama, at its Seventh Annual Session, begun and held in the City of Montgomery, Jan.* 10–12, 1854. 8vo. pp. 190.

Transactions of the Medical Society of the State of Pennsylvania, at its Annual Session, held in the City of Pottsville, May, 1854. Vol. IV. Published by the Society. 8vo. pp. 171.

Transactions of the Medical Association of Southern Central New York, at the Eighth Annual Meeting, held at Homer, June, 1854. 8vo. pp. 120.

Transactions of the Fifth Annual Meeting of the Medical Society of the State of North Carolina, held at Raleigh, N. C., May, 1854. 8vo. pp. 97.

THE annual volumes of *Transactions* issued by the several State Medical Societies we have always looked upon as publications of very great interest. Although the reports embraced in them may be, occasionally, commonplace or superficial; although the facts they record may not always be detailed with sufficient fulness and precision, nor examined with proper care in all their bearings and correct relations; and although, too often, undue space may be given to matters of mere speculation or of little value, while those of primary importance are overlooked or carelessly treated, still, we never fail to find embodied in these *Transactions* observations of great value, calculated to correct, confirm, or enlarge our views in relation to the etiology, character, and proper treatment of the diseases incident to the several sections of our country, or which prevail among the different classes of its population.

1. We have examined with a very great degree of satisfaction the reports and papers constituting the published *Transactions of the Medical Association of the State of Alabama*, at its last annual session; all of them are replete with interest, while several are particularly valuable.

The session was opened by an address from the President of the Association, Dr. A. Denny, "On the Magnitude of Science, or a Contrast between the three Learned Professions." The style of this address is perhaps too rhetorical, as well for the subject discussed as for the character of the assembly addressed. Many of the bold expressions of the speaker are liable to be misunderstood; they will lead some to attribute to him sentiments which he certainly never meant to inculcate.

The annual oration of Dr. A. Lopez is an address of a very different character from that of Dr. Denny. Its subject is an important and interesting one: "The Mutual Relations that should exist between the Representatives of a Commonwealth and its Medical Men;" or, in other words, the necessary co-operation of the sciences of legislation and of medicine for the promotion of the common weal. With commendable dignity, and in clear and energetic language, Dr. Lopez urges upon the attention of legislators the necessity in every State of a body of well-instructed physicians, not merely to minister to such of its citizens as may be overtaken by disease or suffer from accidental violence, but, by their professional knowledge and skill, to aid the legislature in the enactment of laws having for their object the protection of the health and the promotion of the vigour, comfort, and general well-being of the entire community; to assist the officers of justice in the detection of guilt and the protection of the innocent; and to render efficient the efforts of the philanthropist to diminish crime, pauperism, and suffering. In consequence of the important services thus rendered by the physician to the community at large, Dr. Lopez insists upon it as the duty of the legislature to do everything in their power to promote medical education, and to protect the legitimate members of the pro-

fession from the encroachments of ignorant pretenders, and of charlatans of every class and description.

The following extracts will give to our readers some idea of the manner in which Dr. Lopez enforces the claims of the medical profession upon the attention of what should be the ever-vigilant conservator of the people's best interests—an enlightened legislature:—

"The progress of science, and consequently of truth, cannot fail to impress the reasoning man with the conviction, that degenerating influences are reflected upon the intellectual integrity of communities, accordingly as their physical organization improves or deteriorates. Not only intellectual, but also moral character feels the injury which a defective physique imparts. History attests this truth, medical history corroborates the assertion; and the world's chart, with its multiplied statistics, presents daily to every government interested in the inquiry, causes to show how necessary it has become to legislate by acts that 'grasp beyond a grain, and look beyond an hour.' Nor can governments be deaf to the call of reason, or blind to the evidences of political revolution. The bureaus of census and registration, inquiries into sanitary laws, and commissions of health, are daily growing into importance among nations; and the affinity of the parish pauper with the parish sick, create the thought, how much the last two influences the others, as correlative causes of much that bears upon their external as well as internal safety.

"Hence, you must infer that the *people's health constitutes the people's safety.* I desire you to give this a latitudinous construction. It is not alone the health of an individual, but of a generation. It is not an isolated disability, ending with the victim of its attack, but extending through a mysterious series of hereditary transmissibility to every collateral branch of society, in the present, and for the time to come.

"Permit me, therefore, Gentlemen Representatives, to invite your serious thoughts to how much you should be committed in the encouragement and protection of every measure that can rouse up and foster in your State a well-educated, moral, and scientific corps of professional men, to whose skill and care you can confide the well-being of her citizens. Ask yourselves, whether it be be prudent and wise to adopt a policy that retards their advancement, discourages their efforts, and represses the spirit that should prompt them, rather than throw your buckler between them and sinister influences, deeply calculated to operate as fatally upon yourselves and your constituents as upon a profession whose object is exalted, and whose utility is unquestioned.

"There is scarcely an electric flash that pulsates along those winged messengers, momently flying through space, from one continent to another, that does not tell how sensibly alive are the nations of the earth to the great problem of human life. How is its percentage established? what constitutes the bases of your life insurances? or, how test the acumen of your political economists? Surely, by the reliability that you can place upon those who are not only called upon to testify in cases of doubt, but who also have in their keeping the preservation and fortifying of those sanitary conditions by which the citizen is qualified to do service to his government; by perpetuating and transmitting a healthy and vigorous race into her commonwealth; by guarding her against the incubus of pauperism, vagrancy, and mendicity, which drain her resources, disorganize her morals, and disgrace her estate; by preserving intact, through healthy physical organization, the intelligence of her subjects, and filling her stations of power and dignity with moral aliment, rather than crowding her hospitals and jails with the unseen, though certain consequences of depraved and ruined constitutions. Would you understand this?—ill health, with ruined or unsound constitutions, begets inability to labour, destitution, desperation. Upon these follow temptation, crime, contempt of law, insanity, death by self-immolation, and consequently, impoverishment and debility of the body politic.

"This is no fancy sketch, pleading for a profession that has emerged from its long infancy, and now feels its own strength in the consciousness of a well-earned reputation; but figures and statistics verify my statements. They prove that in proportion as a provident legislature invests qualified persons with all things pertaining to sanitary and hygienic regulations—to the extent

that it works consentaneously with the profession most conversant with requisitions for the public health, and upholds its members by its strong arm and liberal interpretation, closing the door against dangerous and untutored men, so far will it accomplish its highest duty, and so much will it enhance the obligation under which it lays its citizens."

"Does the statesman seek to thread the labyrinth of human progress; the lawgiver ask for light upon the dark and hidden phases of natural laws; or municipal power grope in vain for guidance when pestilence and death overshadow the land? Who is it that elucidates and instructs? It is the scientific physician, whose eye no form visible eludes; from whose ear no sound audible escapes; beneath whose hand the mystic law resolves itself into shape, and in whose crucible of untiring exploration, elements apparently indivisible are simplified and distinguished. The forest and the cave, the ocean and the air, things animate and inanimate, yield up their properties at his behest; and their most recondite virtues are by his knowledge brought forth, that man should triumph over pain."

"With what are we not in close alliance wherever the cause of man can be served, his woes assuaged, his condition meliorated? Jurisprudence calls us to its aid; crime can elude the call for justice where the means of detection are confined to legal phraseology, or to human laws, indicating and defining punishment according to testimony. But evidence erected upon human fallibility, discordant statements, the workings of prejudice, revenge, or misconstruction, might fail in justice and in mercy. Innocence might satiate the avenging law, guilt escape its desert, *if there was no surer road to truth*. Under whose touch, then, shall the door of the temple, wherein sits truth enshrined, be opened? Where is the tongue that never lies?—where the accuser and the witness, beneath whose unflinching and irresistible gaze the man of crime stands abashed, and by whose revelations he stands condemned? The *knife*, the *microscope*, the *crucible*—these bring their labours to the courts of justice, and throw them into the common inquest: and yet, the knife of the scientific explorer, which gives to the dead this tongue of truth—the tests of the laboratory, which convert the lifeless mass and its incongruous contents into a living oracle, to confront crime, and pronounce the murderer's doom, are never confided to other hands than ours. It is *medical science that directs that tongue and that eye;* and the *lex non scripta* of the book of nature explains what to the *lex scripta* would be a hidden light."

"The medical man of science allays the wide-spread panic when affrighted cities seek his counsel; and his dictum, like oil upon the troubled waters, calms those hasty outbreaks of popular impulse, so frequently subversive of the best interests of commerce. Social order, in all its bearings, receives the benefit of his judgment, and through his acquirements and moral influence public quiet is insured. But should doubt give way to sad reality, and the dark cloud, portentous with coming woe, threaten to cover the city with its funeral pall, to whom do the people fly for prevention and for preservation? The scientific physician is again at the helm, and dictates the police by which the blow may be either averted, or, if it must fall, shorn of its severity. Passing from the anticipation to the advent of the disease, and you find the medical profession at its post. With it, all distinction is levelled, all appeals equalized. The voice of squalid misery rings the alarm at the portals of its conscience as loudly as the proffered gold of the man of wealth, and as readily is it answered. His benevolence sees no duty save in the exercise of a common charity; his humanity requires no other stimulus than the stricken and the afflicted. For them he braves the lurid heat; he breathes the pestilential vapour; for them he is deaf to the strong appeals of self-preservation, until, alas! too often the shaft averted by his own skill from its fatal destination to another's heart, strikes, in its rebound, the envenomed barb to his own life-blood."

But it is time for us to turn our attention to the reports from the different counties of Alabama embraced in the volume before us. They differ in interest, and in the completeness and value of the facts they communicate; they are all, nevertheless, replete with interest, and will amply repay an attentive perusal.

From the report on the diseases of Line Creek, Montgomery County, we quote the following remarks of Dr. M. G. Merriwether on the treatment of rheumatism. In several very severe cases of the acute form of this disease we have ourselves prescribed, after bleeding, and the full evacuation of the bowels, infusion of the cinchona bark with the most decidedly beneficial results. What Dr. M. says in regard to oleaginous inunction is deserving of attention.

"There were several cases of acute rheumatism. I am satisfied that the quinia treatment is the best possible in this climate. I treat a case of acute rheumatism as I would an attack of bilious remittent fever; that is, so far as regards the constitutional treatment of the two diseases. Calomel and Dover's powder, followed by quinia and Dover's powder, in full doses, will quickly arrest rheumatic fever. That this should be so is not surprising, since it is a well-established fact, that there is not, in the whole materia medica, a remedy so efficacious in subduing inflammation, as calomel in combination with opium, in some of its various preparations." "As a local application in rheumatism, I have the limb rubbed with volatile liniment, with gum camphor—oil of turpentine and laudanum in it—and then have it swathed with flannel. I should like to call the attention of physicians to the importance of oleaginous inunction as a hygienic measure, and to its curative effects in all chronic diseases." "To restore the healthy condition of the skin, there is nothing so good as to have it rubbed with a little oil—animal oils are the best, as they are more penetrating, and do not dry up so quickly. Personal cleanliness is necessary to health, but after a liberal application of soap and water to the body, a little oil should be applied to supply the natural oil of the skin." "The ancient Jews, Greeks, and Romans, who probably excelled modern nations in longevity, applied oil to the skin after every bath. The costly ointments spoken of in ancient history, both sacred and profane, were oils highly perfumed. It would be well for the people of our day to follow the example of the ancients in this particular. It was stated when the cholera was in France in 1832, that no one who was engaged as an oil-porter took the disease. Dr. Simpson, of Edinburgh, states that the operatives in the woollen factories in Glasgow are not subject to cholera and tubercular consumption, owing to the fact that the large amount of oil employed in the manufacture of woollens keeps the hands almost reeking with oil. Dr. Tilt, of England, in an article on hygiene, states that when King Augustus asked a centegenarian how he retained his health to such an advanced age, the latter replied: 'I apply oil to my skin, and mead to my stomach.'

"In all chronic diseases the skin is more or less affected; it loses the natural oil so necessary to keep it pliant, and permeable to the escape of perspiration. In all fevers, also, of a low grade, it is very necessary to apply oil to the skin. In typhoid fever, it is an excellent remedy; it should be applied every day, and every third day the whole surface should be sponged over with strong lye of wood ashes before reapplying the oil; the lye should be warm."

Of the remedial properties of the *veratrum viride* in pneumonia, we find the following favourable account given by Dr. W. H. Anderson, in the report on the diseases of Mobile. Speaking of the prevalent diseases during the first quarter of the year, he remarks:—

"Pleurisy and pneumonia were quite frequent, and the latter was often severe. We cannot say, however, that the mortality was any greater than common." "The veratrum viride, which enjoyed some reputation the previous winter in the treatment of pneumonia, was again tried, and lost none of its laurels. It was regarded by some of our leading physicians as a most valuable remedy, powerful and immediate in its effects, and quite heroic in its nature. It never failed to reduce the pulse to as low a standard as could be desired, and to make an impression on the disease in its active stage highly desirable to both physician and patient. Experience still proved, however, that the utmost caution was necessary in its administration, and that the patients subjected to it should be always under the eye of the physician or of an intelligent nurse. In several instances extreme prostration followed its use in the medium dose, and great apprehension was caused for the recovery of the patient. We believe, however, that it never proved fatal in a single instance. Your reporter admi-

nistered to a child, twelve months old, a single drop, and in the course of an hour, so incessant was the vomiting, and so great the prostration, that he was compelled to resort to a powerful stimulant to prevent the little patient from sinking entirely."

Of the effects of the veratrum viride in typhoid fever, Dr. Anderson speaks as follows:—

"Typhoid fever was more prevalent than it was the previous winter; and, from all accounts, was more easily managed. The veratrum viride did not exercise the influence over this disease claimed for it by some of the practitioners of the interior, and of other States. In the hands of several physicians with us, its effects were not appreciable. Others thought it a remedy of some value, though of doubtful use after the third week of the disease. Your reporter administered it in perhaps a dozen cases, but never pushed it to its full extent. He was satisfied that it often acted beneficially, and would not, with his present experience, like to treat a case of typhoid fever without occasionally making use of the remedy in its minimum doses.

"We doubt very much whether the typhoid fever that occurs in Mobile is accompanied with as much abdominal derangement as appears to be the case in many other places. There is certainly a good deal of meteorism, but the ilio-cœcal region cannot be so gravely implicated as it often is in the Northern States, and on the continent of Europe. We have seen many cases run their course, and end in recovery or death, without any undue tenderness in the right iliac region, and we have examined several cases after death, when no lesion of consequence was found among the patches of Peyer. Another reason that lends value to our position is, that hemorrhages from the bowels are rare, even in those cases that end in death. The degree of *tenderness* on continued pressure in the ilio-cœcal region, is a very good index to the amount of ulceration within, and we should like to know from physicians, practising in those localities where the disease is so deadly, in what proportion of cases there is much tenderness and trouble in that region. We cannot help thinking that the reason why the disease is so seldom fatal here, is because there is so little dangerous ulceration of this portion of the bowels."

Dr. J. W. Crawford, in the report on the diseases of Centerville, presents his experience of the veratrum viride in fevers generally. He states that, during the past year, he used Norwood's tincture somewhat extensively, and, he thinks, with decided benefit in most cases. "That," he remarks, "it will control the action of the heart to any reasonable extent, and this without much inconvenience from its nauseating effects, my experience with it teaches me; and I am satisfied that it also reduces the heat of the surface, in fevers, more quickly than any other remedy with which I am acquainted—cold water excepted—and that, too, without nausea or much reduction of the pulse. I have, also, in three or four instances where it was the remedy relied on, seen the tongue become clean, the mind clear, and all the symptoms improve, the patient becoming rapidly convalescent, where, from observation in such cases of other remedies, I did not expect it. I have not given it in either intermittent or remittent fevers, being fearful of increasing the tendency these always have to gastric irritability. In two cases of fever, one an adult and the other twelve years of age, both proving fatal, I discontinued it after the second dose, owing to the excessive nausea in the case of the adult, and to prostration without vomiting in the child. To the adult thirteen drops, and to the child seven were given, in two doses each. I am not prepared to say that the fatal result was in either case attributable to the remedy—indeed, my conviction is, that it was not; but neither *perfectly* recovered from the prostration which I attributed to the medicine."

The estimate of the remedial virtues of the veratrum viride formed by the authors of other of the reports contained in this same volume of *Transactions* is by no means so favourable as that of the gentleman whose testimony is given above. Thus, Dr. J. C. Marks, in the report on the diseases of Selma, presents the details of an attack of pneumonia treated by the veratrum viride, of which he was himself the subject, and remarks that—

"This case, we think, will show, and equally others which we could adduce

bearing upon the same point, that while it—the veratrum—reduces and controls the pulse—the arterial circulation—and sickens a man, even to indifference of life, it yet, in very many instances, exerts no curative influence over disease—does not remove the essential conditions, whatever they may be."

In the report on the diseases of Sumterville and vicinity, Dr. L. H. Anderson presents some very interesting remarks on the character and treatment of pneumonia, as it occurs in South Alabama. The following are the general conclusions to which he has been led by his personal observations.

"1. The pneumonia of South Alabama is probably less severe than the same disease in more northern latitudes; and the intermittent or remittent fever accompanying it is more dangerous than the pulmonary inflammation.

"2. The treatment should be directed chiefly against the malarious element of the disease. Quinia should be freely given for its antiperiodic effect, and mercury for its influence on the secretions, particularly that of the liver.

"3. An exception to this pathological treatment is to be made when the attending fever is of a typhoid character. Here the dothinenterite is the more important disease, and the pneumonia should be treated with strict reference to it."

The treatment of the yellow fever as it occurred in Alabama during the year 1853, is very fully discussed by Dr. J. C. Marks, in the report on the diseases of Selma. Of bloodletting he remarks—he is speaking of the violent forms of the disease attended by "extensive and dangerous congestion." "Venesection exerted the happiest effects in every case, whether employed by myself or others, in which its use was restricted to the form of the disease under consideration, and the robust constitution. Indeed, we know of no remedy whose immediate and necessary influence over the essential pathological state better fits it for such cases, especially in their early stages; and we confess ourselves at a loss to conceive how such a weapon of successful conflict with so terrible a disease could have fallen into disrepute, save only from an indiscriminate resort to it, in mild or low forms, and weak and enfeebled constitutions, or the failure to follow up and perpetuate its good effects over the congestion by the prompt and judicious exhibition of the *great remedy*, whose action in turn it so much favours."

Dr. Marks here alludes to mercury, some form of which, he remarks, was prescribed in every case; and of all the curative means employed, it was deemed the most reliable. It was customary, he tells us, to precede the use of the mercury, at least when given with a view to its constitutional effects, by cathartics, the warm bath, or bloodletting, or all of these remedies, for the purpose of removing to a certain extent the congested condition of the organs, and thus preparing the system for the prompt absorption of the mercury, and by this means securing its full effect.

"By some," he continues, "the opprobrium has been made to rest upon mercury, that it is incompetent to the relief of aggravated cases; that these will march on to a fatal issue in defiance of the remedy. We believe the source of this stigma to reside chiefly in the culpable neglect of physicians to employ a preparatory treatment, such as will insure its specific action upon the system. Despite that other irrational and illogical dogma which we also now sometimes hear, that even in such cases where the constitutional effects of mercury are obtained, it is more than probable recovery would have ensued without it, we hazard the remark, that there is no remedy known to medical science the rationale of whose therapeutic action so adapts it to the cure of the disease under consideration, especially in its more violent forms. It exerts a powerful influence on the pathological condition, and removes the congestion through an increase of the secretions, and a permanent stimulant impression upon the capillaries. It is evident, then, that the great desideratum must be in the adoption of such measures as will insure its absorption and permanent specific effect upon the system, without which, of course, its use can be of little or no avail. Let physicians studiously seek the attainment of this end, and we must believe that many of the terrors that now wait upon the disease will soon be no more."

"As before intimated, we generally commenced the mercurial treatment, for

its specific effects, soon after the operation of our purgative, or the employment of other preparatory measures, and by the exhibition of calomel in small and divided doses. The quantity prescribed was usually about two grains, combined with from two to three grains of quinia and one-eighth grain of morphia, which was repeated every two or three hours, as circumstances seemed to require. It was continued in this manner until salivation was produced, after which we in no case witnessed anything but the happiest results."

"We did not, in this form of the fever, prescribe the quinia, in the large doses which have been recommended, to cut short the attack, because it was not regarded sufficiently paroxysmal in its nature to allow of such a result. By a few physicians, however, this was attempted to be done with the remedy; but no success of consequence, I believe, was achieved. The morphia, when it failed to quiet and allay pain in the doses prescribed with the calomel, was given in larger quantities alone for that object."

"Cups and blisters were, in the early stages, sometimes employed over the stomach and spine as revulsives, and frequently effected the removal of vomiting, as well as determined to the surface. The cups were occasionally applied to the temples and nucha when the head symptoms became troublesome. Sinapisms were not unfrequently used over the region of the liver, and sometimes applied to the extremities.

"In addition to quinia and morphia, which were employed as stimulants through the disease, in the manner mentioned, it frequently became necessary, in the latter stages, to resort to some of the more direct remedies of this class, and we found ammoniated milk punch and port wine, or the oil of turpentine, with ether, spirits of lavender and camphor water, as recommended in a recent number of the *London Lancet*, to meet as near as possible the indication."

Of the "quinia treatment" of yellow fever, Dr. Anderson, in the report on the diseases of Mobile, speaks as follows:—

"With due deference to the opinion of several highly respectable physicians, who oppose what is called the Quinia treatment, we must give our testimony as decidedly in its favour. This treatment was much used in this city. Your reporter and his associate in practice being much prejudiced in its favour, from the experience of former years, used it from the very commencement of the epidemic to its close. They treated upwards of eleven hundred cases; and, without claiming any superiority in success, they feel sure that their tables of mortality will compare favourably with those of any other physicians in the city. They used quinia in almost every case, regardless of age, sex, idiosyncrasy, or any other circumstance. They have every reason to be pleased with their manner of treatment, and, with their present experience, would not exchange it for any other that they have heard of. The marked and almost magic effect of a large dose of quinia at the outset was so apparent, that they should have considered it little short of trifling with human life to have adopted any other treatment. They will not deny that there were cases in which it did no good; in fact, in those cases where there was at the commencement decided congestion of the brain, it may sometimes have done harm; but such cases were very few, and could hardly have been aggravated by any medicine that could be given."

The quinia was, however, never trusted to alone; it was always given in combination with calomel or blue mass. The quinia, we are assured, had a powerful influence in allaying nausea and vomiting. Very often, on a first visit, the patient would be found with a distressing sick stomach, which was "immediately relieved by a large dose of quinia and calomel."

Dr. Anderson believes that the yellow fever, as it prevailed in Mobile during the past year, was contagious; "as much so as smallpox, or any other disease that we know of."

"Within our own experience," he says, "we could enumerate more than one dozen families, that were isolated from the disease, and escaped until late in the season, but took it within a few days after some transient person came into the family, either sick with the disease, or on the eve of being down with it. If such cases occurred only in one or two families, we might pause and doubt; but they happened so often, and under such circumstances, that we cannot attribute them to anything else but absolute contagion. We have heard the

arguments of others, whose capacity for observation we have the highest respect for; but they have failed to convince us that the fever did not spread in *many* families by contagion." "A family living eight miles from Mobile, in a healthy pine country, has no communication with the city, and remains in perfect health until the middle of October. About this time, a stranger arrives, just from the city; he is seized with the fever, and in a week from that time four or five of the family are taken down, and half the number dead with black vomit."

Startling as these facts may at first sight appear, they cannot be received as conclusive evidence of the contagiousness of yellow fever. To determine their full and true bearing upon the question at issue, there are a number of collateral circumstances to be taken into account, of which Dr. A. has left us in entire ignorance, that, we are convinced, would materially change their etiological import.

Dr. Marks, in his report on the diseases of Selma, bears very different testimony on the question of the contagion of yellow fever to that of Dr. A. He assures us that—

"There was nothing in the history of the epidemic that could, by the most favoured construction, be made to support the opinion of the contagionists; but, on the contrary, everything was presented that could be desired to confute and overthrow it."

"It has long," he further remarks, "been a vexed and contested question, whether, between yellow fever and our ordinary periodic fevers, there be not an identity of cause; or, in other words, whether the same local conditions which engender intermittent and remittent fevers, may not also originate yellow fever. Now, connected with the origin of yellow fever here, there are some circumstances which seem to favour such a view, while other reasons and facts stand in opposition. It is well known that, while yellow fever generally prevails where periodic fevers are common, yet that it occurs only at long intervals or on rare occasions, and is not usually produced when the causes engendering endemic fevers are in operation. If, then, it be produced by them at all, it must be when they become exceedingly active and powerful. Now, in the present instance, before the conclusion of an identity of cause can, we conceive, be logically deduced, we must prove, not only that the ordinary causes of intermittents and remittents, such as malarial influences, hygrometric conditions, &c., were much concentrated and intensified ere yellow fever manifested itself, but, also, that a similar concentration obtained at other places where it prevailed. Should we be incapable of doing this, we are forced to admit a new, and, as we have called it, atmospheric condition, as alone requisite to its production, or at least conspiring with and aiding the common causes of fever.

"We think we may show that there was some concentration of our endemic causes here, at the time yellow fever appeared, as exhibited not only by the greater violence and malignancy of our common periodic fevers, but also from the prolific source of malarial emanations in our midst. We had at that time a general pulling down and upsetting of old filth-incrusted houses, and, besides, a great deal of dirt and decayed vegetable matter thrown up in our streets, especially the one bordering on the river, along which were several large buildings in the course of erection, and to obtain a foundation for which extensive excavations were made; thus, in different ways, exposing to the desiccating and evaporating action of the sun, old pestiferous decomposing matters, which had accumulated in the city for years. Now, while the impure malarial air, in this way engendered, has long been regarded as a fruitful source of disease, and even of yellow fever, yet, as we have every reason to believe the disease occurred at other places where no such agencies, nor even other causes of concentration were present, we cannot conclude that our ordinary causes of fever, heightened as they were, alone produced it, and are, therefore, led to the conviction that an atmospheric epidemic influence of some kind was, if not essential, certainly accessary in its development."

An able and valuable report on goitre, by William Taylor, of Talladega, is replete with interesting facts in regard to the particular localities in Alabama, in which that affection is endemic.

So far as his own observation extends, and so far as he has been able to obtain

information from others, Dr. T. has found goitre to prevail to a much greater extent in the silurian and metamorphic regions of the State, than in other portions of it; and that, while it prevails in the silurian districts to a greater extent than can be readily accounted for by sporadic causes, it prevails to a still much greater degree, in point of frequency, in the metamorphic districts.

"But," remarks Dr. T., "it does not appear that goitre is found alike in all parts comprised in the metamorphic system. In Talladega, the disease does not seem to prevail to so great an extent in the metamorphic region of the country as in some parts of the silurian; while in parts of Randolph County, which is wholly metamorphic, it is becoming so prevalent as to demand the serious attention of the profession. In Fishhead Valley, in Randolph, where the disease is, perhaps, as prevalent as in any other part of the county, much of the water, on standing, precipitates a deposit of mica, similar to what I saw in the water-vessels at Mindereri and Masaya (South America). Yet I do not hold that this agent has any influence, either negative or positive, in producing the complaint, although I have never seen it prevalent, except in the older systems. This fact holds true, so far as my own limited observation extends on this continent, and I doubt not but the same holds with regard to the old world. I have interrogated several intelligent Englishmen in regard to the goitrous districts of England, all of whom tell me that the shires of Nottingham, York, and Derby are metamorphic in their geological aspect, and abound in mica. But if, as M. Chatin reasons, goitre is dependent on the absence of iodine in the water, then the absence or presence of mica becomes a mere adventitous coincidence. Indeed, I would not pretend to assert that mica had any direct agency in producing goitre, but merely mention it in connection with the disease; because I believe there is as much ground for assigning it as the cause of the malady, as there is for attributing it to lime, magnesia, and divers agents, to whose influence it has, at different times, been charged. And if future observation and experience should confirm the suggestions and reasoning of M. Chatin, then will goitre and the more primary systems be found to have a relative connection, because little iodine is found in these systems, while it is more liberally diffused throughout the more recent formations. In other words, we may expect to find the disease confined principally to the older formations; while, on the other hand, it will rarely be found occurring in systems of more recent origin."

"From facts, the causes of which are as yet unexplained, there are reasons for supposing that the metamorphic formation offers a more prolific field for the production of goitre than the purely primitive; since it is a remarkable fact that countries where goitre prevails to any considerable extent, are decidedly metamorphic. In the central Swiss Alps, notorious throughout the world for the production of goitre and cretenism, Sir Charles Lyell asserts, 'the primary fossiliferous, and older secondary formations disappear, and the cretaceous, oolitic, and liassic strata graduate insensibly into metamorphic rocks, consisting of granular limestone, talc-schist, talcose gneiss, micaceous schist, and other varieties.' This condition of things exists in all the goitrous districts which we have visited, and we should hesitate but little in venturing the opinion that the same holds true in every part of the world where goitre is known."

The following are the general conclusions at which Dr. T. has arrived, from the facts and reasoning set forth in his report:—

"1. That goitre exists, in some sections of the State, to the extent of about *one per cent.* of the population, and will, in time, unless proper measures are adopted to prevent it, assume an endemic form.

"2. This disease being confined principally to the older geological formations of the State, it will, in all probability, never prevail to any considerable extent in the more recent systems.

"3. That the cause of goitre is pretty certainly traced to the character of the water used in the infected districts.

"4. There is strong reason to believe from the observations of M. Chatin, and the scarcity of iodine in goitrous districts, that the disease is not produced by any positive agent, but has its origin from the want of a due amount of iodine in the waters of said districts.

"5. That goitre is not found in regions where the waters are impregnated with an appreciable quantity of iodine; hence it will never exist, to any considerable degree, in the tertiary and cretaceous systems, nor the coal meadows of our State, since iodine is abundantly diffused throughout these formations.

"6. Iodine is the remedy, in the treatment of goitre; and this fact, under the circumstances, is strong corroborative evidence that the disease originates from the want of that agent; and if the absence of iodine is the cause of bronchocele, it is but reasonable to expect that great good may be obtained by impregnating with it the water used as drink, as a prophylactic measure in goitrous districts. May not the remedy in time become as certain a preventive of goitre as vaccination is of smallpox.

"7. Iodine does not seem to form any chemical combination with the blood, nor with any of its elements; but seems to impart something to the system—the lymphatic system especially—necessary to the maintenance of its healthy functions.

"8. Surgery has done but little for the relief of the goitrous patient. Extirpation has been attempted, with unfavourable results; it is now totally abandoned by the profession. The seton has been resorted to with limited success. The formation of an abscess in the tumour has, in some instances, resulted in a spontaneous cure; tying the thyroidal arteries has also been attended with partial success.

"9. Certain gum resins are applied externally in South America, and also in some parts of the republic of Central America, with reputed efficacy."

Besides the regular reports from the several counties of the State; that on goitre, noticed above; a report on the botany of Wilcox County; reports on the number and character of the physicians of Dallas and Bibb counties, the volume of *Transactions* before us contains an account of a case of distorted pelvis, in which abdominal section was practised, followed by death from nervous shock; another of leucorrhœa in an infant twenty months old; a paper on the unity of disease, by Dr. H. Backus, and one on the pathological relations of the organic nervous system, by Dr. M. Troy. These last two, though highly creditable productions, and not devoid of a certain degree of interest, are out of place, in our opinion, in the *Transactions* of a State Medical Society, the communications to which should be confined, as much as possible, to a record of the personal observations of its members on the several questions connected with the pathology and treatment of the diseases prevailing in their respective localities, whether as endemics or epidemics, with a comparison, it may be, of their own observations and conclusions with those already recorded by others.

2. The *Transactions of the Medical Society of the State of Pennsylvania*, at its annual session of 1854, in their intrinsic value and general interest fall far short of those of former years. It is with surprise and deep regret that we perceive that in so small a portion of the counties the medical profession is represented at the sessions of the State Society, and that of those represented, so few present full and satisfactory reports, as a contribution to the common stock of medical knowledge. To what cause this apathy to the best interests of the profession, this neglect to perform what should be considered a pleasing task as well as a positive duty, is to be attributed, we pretend not to determine. It is to be hoped, however, that efforts will be made to effect, at an early date, a complete organization of the regular physicians in every county in the State; and that, when such an organization is accomplished, a full representation from each county will be found in attendance at the yearly meetings of the State Society, prepared to report on the character, causes, and usual course of the diseases that occur in the several sections of country included within their respective limits, and the plans of treatment which experience has shown to be most successful in conducting them to a favourable termination. A very common excuse offered by physicians residing in the interior of the State, remote from any large town or densely settled neighbourhood, for their neglect to become active members of the medical society of their county, is the little leisure which the hard labour

and constant travelling of a country practitioner leaves him, even to attend the meetings of the Society, much less to communicate to it in writing, the results of his observations and experience in relation to the several cases of disease which fall under his notice. That in some cases, the country physician may not be able, without a very great sacrifice, or very considerable inconvenience, to attend regularly the meetings of his county society, we can readily suppose;—we must, however, insist that the advantages to be derived from such attendance, will, in nearly every instance, amply repay the sacrifice and inconvenience it occasions. But we cannot admit the plea of want of time and opportunity as a valid excuse, on the part of any physician, for not recording his personal observations on the diseases he is called upon to treat, and his experience in reference to their proper treatment. Short, pertinent, and accurate notes are all that is required; for the recording of which an opportunity may be found amidst the occupations and cares of the most extensive and wide-spread practice—without interfering in the slightest degree with the duties the physician owes to his patients, or encroaching upon the time necessary for his rest and refreshment. Were such notes to be furnished, by even a majority of the members of a county society, to the committee appointed to draw up its annual report to the State Society, we should no longer be under the necessity of hearing the constantly recurring and mortifying apology for the incompleteness of these reports, the want of the materials requisite to furnish a complete and accurate medical history of the county for the years they embrace.

The annual address by the president, Dr. John P. Hiester, of Reading, is on the means of suppressing the evils of quackery.

We entirely coincide in opinion with Dr. Hiester, that these evils can never be totally eradicated, even under what may be considered the most efficient means, and by the wisest and most persevering efforts. All that we can reasonably expect to attain, is to bring the evil under wholesome restraint.

One of the leading means for staying the evils of quackery in our midst, is the cultivation by the profession of everything calculated to increase its legitimate influence with the public. By the proper organization of its members, with the view of insisting upon the moral and educational qualifications of all who would enter its ranks, and their strict adherence, subsequently, to a code of ethics, clearly defining their duties to the community generally, to their patients, and to each other. Let the entire profession raise itself to its proper standard in education, scientific attainments, and skill; in moral worth and gentlemanly deportment. Let its members be truly united among themselves; banishing all petty jealousy, and every mean art of rivalry, and prepared mutually to defend each other's rights and interests, and a more effectual barrier we shall have erected against the progress and encroachments of quackery than can be expected from penal enactments, the shafts of ridicule, or the most convincing arguments addressed to the public ear.

We agree with Dr. Hiester, as to the beneficial results to be anticipated from a general diffusion, through the medium of our public and private schools, and by popular courses of lectures to the members of the several institutes and lyceums of our towns and cities, of a knowledge of the structure and functions of the human body, and the laws of health and disease deducible from such knowledge; though we are not so sanguine as he is, that by this means a "radical remedy" to quackery will be put in operation. The adaptation of therapeutic agents to meet the various and varying conditions of disease, can never be understood by those out of the profession; and the cunning, intelligent quack, and we have, unfortunately, such in our midst, will base the success of his nostrums upon this very ignorance, let a general knowledge of anatomy, physiology, and pathology be ever so widely diffused.

The reports contained in the present volume of *Transactions* bear testimony to the very general exemption from disease, especially of an epidemic character, enjoyed by the entire State.

Although the question of the relationship of particular diseases to different geological formations has, as yet, not been examined with any degree of care, yet, so far as any notice is taken of it in the reports before us, it is very evident, in relation to the dysentery, at least, that it may prevail to as great an extent

upon the limestone, as on the gravel, or old red sandstone, contrary to the inference derived from the statements presented in some of the reports of former years.

The diseases that prevailed most extensively during the year 1853, in those portions of the State from which reports are presented, were dysentery, typhoid fever, rheumatism, mumps, hooping-cough, and erysipelas.

In Logan's Valley, Blair County, Dr. T. M. Confer reports the prevalence of mumps to have been more extensive than for many years previously.

"Its first appearance was in the early part of December, and, by the end of the month, some two hundred persons had suffered from it. These were generally the young, between the ages of two and thirteen years. Some few cases were met with in adults. It was carried from the public schools, and so generally distributed, that every family in the township was visited by it. In three cases that I noticed, the glands were suppurated. This process was confined to the superficial integuments, and over not more than one gland in the same patient. Many of the cases were attended with violent fever and delirium, continuing for two and four days, and requiring an active antiphlogistic treatment. Several experienced a second attack, though with less violence than the first. These were adults, having large families, and being constantly exposed. Some few cases of metastasis were noticed; three to the testes, and one to the brain. This latter case occurred in a scrofulous subject, who had glandular enlargement previous to the attack of parotitis. The inflammation passed gradually from the salivary glands to the mucous coat of the eye on the same side, then to the opposite eye, and finally to the brain; when it terminated fatally in five days and a half, notwithstanding every application was made use of to invite back the disease to its original location, and to arrest its progress in the brain.

"In one of the cases of metastasis to the testes, suppuration took place, and by the neglect of the patient to make known his real situation to his parents, the left testicle was destroyed by the suppurating process." "When first seen, the part was discharging profusely, and the suppurative process, for a time, seemed to threaten the extinction of the opposite testicle. This condition of things finally yielded to an appropriate treatment, and the patient was restored to health, with the loss of one testicle."

"The suppurating process in parotitis, which, we believe, is of rare occurrence, is generally superficial, seldom extending further than the integuments, which must render this case one of singular interest; yet we believe it may be accounted for by the exposure of the patient to cold, and the fatigues of labour, the friction of his clothing, combined with the accumulated filth, acting as constant sources of irritation to the inflamed gland."

In the Report from the Medical Society of Berks County, we find the following statement:—

"Dr. Greis mentioned, at the last meeting of our County Society, that he regards erysipelas as almost entirely a constitutional affection, and that he so treats the disease; after having evacuated the bowels with a mercurial purge, he restores the secretions by the use of the *ammoniated tartrate of iron*, in the dose of grs. viij, every four hours; and if much prostration exists, every two hours. From its ready solubility and palatable taste, it forms an excellent preparation for children, and, according to the doctor's experience, is more efficacious than the muriated tincture of iron."

A most interesting communication occurs in connection with the report of the Medical Society of Montgomery. It presents the detail of a number of cases of erysipelas occurring in children subsequent to vaccination, by Dr. Hiram Corson. These cases are certainly of a very strange character. Several of them, in connection with the local disease, which made its first appearance in the neighbourhood of the part at which the vaccine virus had been inserted, presented a series of symptoms indicative of a serious affection of the brain and upper portion of the spinal cord. Unfortunately, no *post-mortem* examination was made in either of the fatal cases. Of the true cause of the erysipelatous attack in Dr. Corson's cases we can form no accurate opinion. Excepting, perhaps, as a mere exciting cause of disease in subjects otherwise predisposed, we cannot suppose the vaccination to have had any agency in its production. Thirty-one children

in the same neighbourhood were vaccinated with the same matter. Of these, eight only were attacked with erysipelas; and of these eight, five died.

"The last fourteen of the children," remarks Dr. C., "were vaccinated on the mild days preceding twelve days of northwesterly wind, which occurred in March last, beginning on the 25th, and continuing without intermission, until the last of April. This was a remarkable spell of weather; the wind never shifted, but continued steadily to blow for twelve days; was very dry and cold.

"On the very day on which this change to cold began, one child, then only vaccinated two days, was seized with convulsions. On the next day, or rather on the night of the second day, three others sickened, of whom two died. On the next day, another sickened in the morning, and died at 11 P.M. On the following day, two others began to be affected with erysipelas of the arm; one of whom died at the expiration of two weeks from the time of vaccination, and all sickened during the continuance of the cold weather.

"At the time those children were vaccinated, I was attending a woman who had been ill with erysipelas since the 14th of February, supervening on a small injury of the toe-nail, and which had involved the foot and leg almost to the knee, when I was called in; and finally involved the whole thigh, suppuration taking place deeply, throughout the whole limb; and it is only now, May 1, fairly free from the discharge of matter. This was the only case of erysipelas within my town until April 15, and was one mile distant from the nearest of these cases, and two miles from many of them.

"On the 3d of April, a child next door to two of the patients, aged nine months (female), was scalded on the arm, and slightly on the neck. On the 14th, those scalds were nearly well, and erysipelas began to show itself on the very crown of the head, from whence it spread rapidly, involving, in two or three days, the whole scalp and nose, with portions of the cheeks. Used mercurial oint. and mur. tinct. of iron. Recovered. I also attended several other small children, in the region about, suffering with slight catarrh, with thin, green, watery stools. But all of them recovered save one very small, weak twin, four and a half months old.

"I have been informed by my friend and neighbour, Dr. Samuel Smith, that, during the illness of these children, he was called to a child of only a few months old, with sickness, green stools, and other symptoms similar to those which existed in the small children mentioned by myself; that in four or five hours after the invasion of sickness, erysipelas showed itself on the point of the nose, and spread rapidly; and the child died in nine hours from the time of its attack, with symptoms of disease of the brain. This case was two miles from any of mine.

"That the matter was pure and good, I have not the least doubt; for a great majority of those vaccinated took the disease well. In two instances, when two children of the same family were alone vaccinated, in each case one took the erysipelas. The disease was severe almost always in proportion as the child was young, and milder as it had more age; and yet, some of the quite young ones escaped, whilst some of even five and six years took the disease."

While the majority of the reports embraced in the present volume of *Transactions* contain much matter of an interesting and valuable character, the most complete and satisfactory of the series is unquestionably the one from the Medical Society of the County of Philadelphia. We have, however, already extended the present notice to too great a length, to permit us to enter upon an examination of the facts and observations it presents in relation to the more prevalent diseases of the period it includes.

3. The volume of *Transactions of the Medical Association of Southern Central New York* contains the history of a few cases of disease which are not devoid of interest, but no report presenting any prominent facts and observations demanding especial notice. But two reports, properly speaking, appear, in fact, to have been made at the session whose transactions are here detailed; namely, one from the Committee on Surgery, from Chemung County, and the other from the Committee on Endemics and Epidemics, of Tompkins County; neither of them very full or satisfactory.

The annual address by the President of the Association, Dr. Frederick Hyde,

is a creditable production, and appropriate to the mixed audience before which it was delivered. Its main topic is the importance of every one acquiring a correct knowledge of the human physical organization, and the vital laws by which that organization is governed; in order that each individual may be enabled to pursue a plan of treatment, which shall encourage a healthful development of all its powers, and preserve them in their full and vigorous exercise for the longest period, thus securing his or her own comfortable existence, and the transmission of a perfect organism to his or her offspring. In the close of the address, are set forth the paramount claims of legitimate medical science as carried into practice by the well-instructed and conscientious physician to public confidence and protection. Both themes are judiciously treated, and strongly enforced.

An Essay on Opium and the Sulphate of Quinia as Remedial Agents, by Dr. Nelson Nivison, demands but a passing notice. To believe the writer, these two articles, in varying doses, either separately or conjointly administered, and appropriately timed, are of themselves adapted to the arrest of fevers, inflammations, and a host of other diseases—that there is scarcely, indeed, "any species of disease in which, either primarily or secondarily, in some stages of its progress," either one or both of these remedies cannot be made to subserve important purposes.

All that Dr. N. advances, in general terms, may be strictly correct, and yet we fear that, neither in principle nor in practice, the views he inculcates in the essay under consideration, will be found to advance in any great degree our acquaintance with the therapeutical application of either of the remedial agents indicated.

An Essay on Medical Education, by Dr. Daniel Holmes, breathes the true spirit, and utters correct sentiments on this all-important subject. A subject we are pleased to find that is attracting the notice of our State and local societies—for it is through their influence and co-operation alone, that any reform in medical education can be effected. Let the members of the profession throughout the Union unite in the demand for the requisite qualifications in those who enter into its ranks. Let them carefully point out to their pupils the nature and extent of those qualifications, and conscientiously aid them in the means for their attainments, by personal instruction, and by affording them every opportunity for the study of disease within their reach, and finally, when prepared to profit by a course of medical lectures, preparatory to graduation, to indicate to them the institution best adapted to complete the thorough course of instruction commenced under the private preceptor's guidance. Let this be done, and the desired reform in medical education will be speedily accomplished; but not until then.

Dr. Holmes presents some pertinent remarks on the deficiencies of the clinical instruction afforded to the medical student in this country.

4. At the Session of 1854, of the *Medical Society of the State of North Carolina*, the subject of the annual address by the President, Dr. J. B. Jones, was, "The manner of man's relation to some of the many destructive agents by which his existence is often terminated." His views on this subject will be understood from the following paragraph, which occurs at an early part of the address:—

"But for his nervous system, and the connate objects of its control, man would escape the injurious or fatal action of all that class of agents from which the vegetable world suffers no inconvenience. This fact plainly declares that nerve is the medium through which he is placed in relation with by far the majority of morbific causes. We read accounts of primary lesions of the blood, secretion, &c., in which accurate descriptions of these changes are given. But if the blood is a vital fluid, as dependent upon innervation for its normal properties, as nerve matter is upon blood for stimulus and supply, a lesion of innervation is a necessary element in the definition of lesions of the blood."

"Poisonous and morbific agents," according to Dr. Jones, "produce death or disease by their action primarily upon the nervous system." Before he closes he admits, however, that the action of all morbific agents cannot be accounted for in this manner. Thus, he says:—

"Any substance entering the blood, and impairing or destroying the integrity of one or more of its constituent elements, impairs, or robs, to a corresponding extent, the organs which are dependent upon such product for their existence and functional capacity; and this is no doubt the mode of relation between individual structures and some poisons and therapeutic agents."

The second article in the volume, is a long address in opposition to the commonly received doctrine of malaria as a cause of disease—chiefly, however, in reply to some strictures of Dr. Satchwell, on a former address on the same subject, by Dr. Charles E. Johnson, the author of the present article.

The address, though in many respects an able one, presents no new facts or arguments bearing upon the subject discussed, and is not, in our opinion, suited to the *Transactions* of a State Medical Society. Positive contributions to medical science, derived from the personal observations and investigations of the members of the several county societies, digested and arranged by competent committees, are what we look for in these *Transactions;* and we are very properly disappointed, when we find, in place of these, theoretical disquisitions leading to no positive results.

A short letter from Dr. James J. Philips follows, On the importance to the physician of the study of the standard medical works that issue from the press, and of the valuable contributions to be found on the pages of our professional journals.

The volume closes with "An account of some of the early proceedings of the Society, and the Code of Ethics of the National Association." D. F. C.

Art. XVI.—*Reports of American Institutions for the Insane.*

1. *Of the Mt. Hope Institution*, for the years 1849, 1850, 1851, 1852, and 1853.
2. *Of the Vermont Asylum*, for the years 1852 and 1853.
3. *Of the Maryland Hospital*, for the year 1852.
4. *Of the South Carolina Asylum*, for the year 1852.
5. *Of the Louisiana State Asylum*, for the years 1849–50, and 1850–51.
6. *Of the Butler Hospital*, for the year 1853.

1. The Mount Hope Institution, near Baltimore, is the largest and one of the most successful of what can properly be termed the *private* establishments for the insane, in the United States. The principle upon which it is conducted, as represented in the report for 1849, is truly benevolent. It "has never obtained from the city or State the smallest pecuniary assistance, or from individual benevolence the slightest endowment," yet, "whilst burdened with a heavy debt, it appropriates one-fifth of its room to the maintenance of destitute lunatics." Persons of all religious persuasions are admitted, and participate "on an equal footing in all its benefits."

	Men.	Women.	Total.	
Insane patients, January 1, 1849	21	38	59	
Admitted in course of the year	45	41	86	
Whole number	66	79	145	[1]
Discharged, including deaths	38	32	70	[1]
Remaining, January 1, 1850	26	48	74	[1]
Of those discharged, there were cured	19	13	32	
Died	8	6	14	
Cases of *mania-à-potu*, January 1, 1849	3	0	3	
Admitted in course of the year	77	11	88	
Discharged, recovered	75	11	86	
Died	3	0	3	
Remaining, January 1, 1850	2	0	2	

The special condition resulting in death, in the cases of insanity, is not mentioned.

[1] There are some discrepancies in these figures which a reviewer cannot correct.

Three cases of what is termed "febrile delirium" were received, and all terminated fatally. We are bound, of course, to *assume* that these were what the term imports under which they are recorded, but since many cases of a disease which is now generally believed to be distinct from ordinary febrile delirium have been reported, at various institutions, under the different terms "typhomania," "phrenitis," "exhaustive mania," "Bell's disease," &c., and, particularly, since Dr. Stokes gives the following description of his cases, we cannot forbear to *presume* that some of them belonged to the form of disease mentioned. "In these cases," says he, "there appeared to be a kind of inflammation of the membranes of the brain, accompanied by a general and intense impairment of all the faculties of the mind. This is a condition, often, we think, mistaken for insanity. *It usually assumes the form of maniacal excitement, sometimes preceded by a short period of melancholy, or depression of spirits. Patients affected by it are remarkable for the rapidity of their movements, incessant talking, and sleeplessness.* The pulse is frequent, but not hard or full, the tongue is coated, and the eyes often slightly suffused. Such cases often prove fatal very suddenly, and after death the pia mater and the arachnoid membrane will be found considerably more vascular than natural, and occasionally some effusion of serum will be observed. *Death does not seem to result from inflammation, but from the exhaustion and sinking of the vital powers after long-continued excitement, agitation, and sleeplessness.*" Yes; and hence it has been found, in most of the northern asylums, that the pretty free administration of stimulus, as wine or brandy, is the only method by which death, in these cases, can be averted, and the patient restored to health. The Italics in the quotation are ours. Some of them denote symptoms, which, to us, do not appear fully to correspond with those of ordinary febrile delirium.

Of the one hundred and forty-five insane patients, ten had the suicidal propensity. "For a long time during the spring and summer, almost every patient brought to us was represented by the friends to be strongly suicidal. None succeeded in carrying their plans into execution.

"The mode of self-destruction in asylums is, for the most part, by hanging. The particular way, however, generally appears to be a matter of much thought and consideration. But, after the plan is once settled, they seem to neglect all other means which may offer themselves, until they have an opportunity of perpetrating the deed in that particular way.

"There is no subject connected with the history of insanity, in which more crude, ignorant, and mistaken notions are entertained, than in what is termed *religious madness.* * * * * The best authorities on the subject declare that there is reason to believe that the number of persons who become insane through the influence of religious hopes and fears, is much less than is generally supposed. As far as our own observation goes, we have never yet succeeded in tracing any one case unequivocally and directly to this cause. * * * * * * The circumstance that the mind of the lunatic is occupied, during the period of his disease, with ideas and feelings connected with an invisible world, is no proof whatever that the derangement of his understanding was produced, in the first instance, by impressions related to the same subject."

From the report for 1850:—

	Men.	Women.	Total.
Insane patients, January 1, 1850	26	48	74
Admitted in the course of the year	32	40	72
Whole number	58	88	146
Discharged, including deaths	33	32	65
Remaining, Dec. 31, 1851	25	55[1]	80[1]
Of those discharged, there were cured	16	15	31
Died	3	5	8
Cases of *mania-à-potu* at beginning of year	2	0	2
Admitted	59	11	70
Discharged cured			65
Remaining at end of year		2	2
Deaths (inferred from the above—not stated in report)			5

[1] Deducted from the preceding figures, these would be 56 and 81.

Several pages of this report are devoted to the purpose of the prevention of insanity, by pointing out the symptoms, and suggesting the proper treatment of the incipient stage. The psychic symptoms are included under the general terms of *changes in the disposition, temper, and affections.* The writer then proceeds as follows:—

"There are certain physical symptoms which will serve to point to the nature of the threatened mischief, and guide the treatment. The patient complains of a sense of tightness or constriction across the forehead, sometimes attended by noise in the ears, flashes of light, flushing of the face, &c.; by a state of watchfulness by night, and restlessness by day; by costiveness, by gastric and hepatic derangement. The inability to sleep is a symptom which ought never to be neglected. It may be considered one of the most valuable indications we possess of approaching insanity. It seldom deceives. Whenever this state of watchfulness by night and restlessness by day occurs in connection with some of the foregoing manifestations of an altered state of the feelings and affections, not another moment is to be lost. Now it is that much may be done by the ordinary family physician. He ought carefully to scrutinize the state of the general functions, and immediately set about rectifying morbid action wherever located.

"Accumulations in the alimentary canal must be removed by purgatives; the secretory and excretory organs must be duly regulated by the warm bath, and by diuretics. Cerebral irritation, and turgescence or morbid excitement of the capillaries of the part, must be subdued by local depletion, leeches to the temple, cups on the nape of the neck, &c. Sleep must be restored by the cautious administration of Dover's powders, or morphia, at night. But on no account should they be resorted to until preliminary steps are taken to subdue the lurking mischief in the brain.

"As soon as possible after these points have been attended to, the patient should be sent on a journey, in order to divert the mind, and to change the whole current of thought."

Theatrical entertainments, or "exhibitions," have been introduced as a part of the amusements of the patients at Mt. Hope. "Many who take no part in these performances, are amused by witnessing them, and imperceptibly get their feelings enlisted in the success of others. By such recreations as these, we have known, in a considerable number of cases, melancholy feelings and insane delusions to be dispelled, and recovery to take place."

In view of the prevalence of delirium tremens, and of the influence of intemperance in the production of other forms of mental derangement, Dr. Stokes urges the importance of legislative action for the reformation and control of inebriates. He would have them removed to houses of refuge, where they should be subjected to medical treatment and moral discipline, the term of their detention to be limited only by the decision, in each individual case, of a proper medical or judicial tribunal.

Report for 1851:—

	Men.	Women.	Total.
Patients at the commencement of the year	25	55	80
Admitted in course of the year	50	52	102
Whole number	75	107	182
Discharged, including deaths, and one escape	38	46	84
Remaining at the end of the year	34[1]	64[1]	98
Of those discharged, there were cured	12	17	29
Died, including one suicide	8	6	14
Cases of *mania-à-potu*, January 1, 1851		2	2
Admitted	53	1	54
Discharged cured			52
Died			2
Remaining			2

[1] According to the preceding data, these should be 37 and 62. In one place the deaths are reported 8 males, 6 females; in another, 9 males, 5 females.

Of the insane patients, two died of acute meningitis, and two of febrile delirium.

In the table of causes, seven cases are attributed to "defective education;" and from the remarks upon this subject we make the subjoined extract:—

"In most of these cases, we are constrained to say, the too fond parents have unconsciously sown the seeds of this most bitter fruit. Too great indulgence in childhood, and previously to, as well as during puberty, and a want of moral discipline then, and up to maturity, with neglect of that education which inculcates, and indeed imposes proper principles of feeling and action, are undoubtedly among the most deeply-laid foundations of insanity. Persons thus brought up have their temper, emotions, and moral affections so little under command—are so subject to ebullitions of passion, to capricious or violent and fugitive emotions—are so liable to act from momentary feeling and impulse—as to acquire a disposition of mind not only most unamiable in itself, but also most prone to marked disorder when subjected to its more immediate productive causes. * * * * In young girls, even though there be no predisposition from a constitutional taint, faulty education will itself originate and develop that exalted degree of nervous susceptibility and irritability which, in later life, may eventuate in *moral insanity*."

The doctor thus gives his opinion in regard to depletion:—

"To bleed and otherwise deplete copiously for insanity alone, even in its early stages, is a highly injurious practice. Without having the least influence over the delusions of the patient, it lowers the grade of his vital forces, and renders him less able to support the more or less wearing influence of the mental disease on the bodily health. * * * * It greatly disposes to a more rapid decline of mania or monomania into dementia. * * * * It diminishes the chances of subsequent cure, and very generally serves to protract the duration of the disease. In our estimation, depletion is the great error in the primary treatment of insanity. * * * * We could point out more than one case now in this house, on whom the detraction of blood, injudiciously resorted to, has inflicted irreparable mischief."

Report for 1852:—

	Men.	Women.	Total.
Insane patients, January 1	34	64	98
Admitted in course of the year	41	42	83
Whole number	75	106	181
Discharged, including deaths	44	42	86
Remaining, December 31	33[1]	62[1]	95
Of those discharged, there were cured	19	23	42
Died	3	5	8
Cases of *mania-à-potu*, including two at beginning of the year	64	8	72
Discharged recovered	58	7	65
Died	5	1	6
Remaining, December 31	1	0	1

"During the last year," says the report, "a larger number than any previous year have been admitted in a state of raving madness, attended with excessive excitement and violence. During the continuance of the maniacal paroxysm, the object aimed at in the medical treatment has been to calm undue nervous susceptibility and irritability, without impairing the vital energy, or exhausting, by reducing remedies, the vital powers. No case of acute mania has occurred in which any depletory measures have been adopted, either by bleeding or other antiphlogistic remedies, save aperients and diuretics, with moderate local depletion, which are generally required in order to remove vitiated secretions, to clear the eliminatory organs of obstructions, and to lessen local congestion. Indeed, it is now an admitted axiom by physicians of insane asylums in general, that in this malady *inflammatory action* in the brain is of exceeding rare occurrence. It would seem as if all the energies of the constitution were directed to

[1] Thirty-one and sixty-four, if the antecedent numbers are correct.

the full development of the mental disorder, and diverted from the processes whereby inflammatory action is established and maintained. * * * * We have invariably endeavoured to support and succor the constitution, and thereby to fortify it against the debilitating influence of long-continued excitement, agitation, and raving. * * * * Cases continue to be admitted in which general depletion has been lavishly resorted to."

Dr. Stokes gives us a table of the ages of the patients, by which it appears that there were 39 between 20 and 30 years, and 69 between 30 and 40. He refers to the assertion that in Europe the predisposition to insanity is greatest in the decennium from 30 to 40 years, and remarks that "in France, it appears certain that persons are more liable to insanity between 30 and 40 years of age than at any other period of life." He quotes from the statistics of the Pennsylvania Hospital for the insane, where 44.87 per cent. of the patients were between 20 and 30, and only 18.9 per cent. between 30 and 40; those of the Ohio Asylum, where there were 43.97 per cent. between 20 and 30, and but 24.52 per cent. between 30 and 40; and of the Bloomingdale Asylum, where the number between 20 and 30 was "much larger" than in any other decade. He then remarks that "the inference to be deduced from our table is *more in accordance with the results of the asylums in France*, inasmuch as the largest number *appear to have been attacked* between 30 and 40 years of age." Let us ask Dr. Stokes this question: *Does* your table show the ages of your patients at the time of the *first attack* of insanity? If it does, your institution presents a most remarkable contrast, in this respect, to all others in the country. It is a perfect anomaly. Your table has this caption: "*Showing the Ages of Insane Patients from January* 1, 1852, *to January* 1, 1853." The reader would hence infer that it exhibits the *present age* of all your patients (for they are all included) irrespective of the number of years they had been in the asylum—regardless of the prior duration of the disease. In France, and generally in other parts of Europe, the ages upon which the calculation is based are those at the time of admission of the patients. Supposing that your table represents the ages in 1853, it is not remarkable that the results should coincide with those of the French asylums, rather than with those of the three American institutions mentioned, where the computation was based upon the *ages at first attack*. Have you not attempted to compare things which, from their very nature, are not susceptible of comparison?

Report for 1853:—

	Men.	Women.	Total.
Insane patients, January 1	33	62	95
Admitted in course of the year	48	72	120
Whole number	81	134	215
Discharged, including deaths	36	47	83
Remaining, December 31	45	87	132
Of those discharged, there were cured	15	15	30
Died	8	10	18
Mania-à-potu cases, including 1 from 1852	20	1	21
Discharged cured	18	1	19
Died	2	0	2

From the tables of the "civil condition" of the *mania-à-potu* patients in the five reports before us, we deduce the following results. Of the men, there were *single*, 138; *married*, 135; *widowers*, 4. Of the women, *single*, NONE; *married*, 27; widows, 8. There is a slight error which we cannot correct, in these aggregates, arising from the fact that, in the course of the five years, *seven* patients remained in the asylum over the 1st of January, and are consequently reckoned twice. But this does not materially affect the results.

"During the past year, the demand for accommodations for the insane has been so pressing as to exclude *mania-à-potu* cases almost entirely. For the last six months we have been unable to admit a single case of it."

In this report, the first of the five in which it is introduced, we find a record of the causes of death of the insane patients. "Three died of recurrent apoplexy, four of exhaustive mania, two of inflammation of the small intestines, two of epilepsy, one of marasmus, one of phthisis, and two of dysentery."

In regard to insanity as coincident with age, the table in this report "corroborates the inference" which Dr. Stokes thought himself "warranted in deducing from the statistics of last year," because 57 patients were between 30 and 40 years old, and only 47 between 20 and 30. Now let us understand ourselves in regard to this subject. If Dr. Stokes would merely assert that at any given time, to-day, for instance, the number of insane persons in our asylums, or in the whole population, between the ages of 30 and 40 years, is greater than the number between the ages of 20 and 30 years, we grant that his assertion is correct. No one doubts it who has examined the subject. If he would assert, or infer, that either at the time of *first attack* of insanity, or of *admission* into the asylums, the number of patients between 30 and 40 years of age exceeds that of those between 20 and 30, we would answer that we have abundant proof that such is not the fact.

In an article published in the *New York Medical Journal*, some six or seven years ago, this subject is discussed probably more minutely and analytically than by any author whose writings have been examined by Dr. Stokes. It includes the statistics of most of these existing American Asylums, and exhibits the ages as taken in four different methods.

1. *Ages of the patients in the asylum on a given day.*—The recovered of but one asylum is given. Result: Between 20 and 30 years, 18; between 30 and 40 years, 32—similar to that of the Mt. Hope statistics.

2. *Ages of all the patients during a given year, including the old residents and those admitted.*—Statistics from four institutions. Whole number of patients 848. Between 20 and 30 years of age, 214; between 30 and 40 years, 245. Corroborative of the Mt. Hope table.

3. *Ages at the time of admission into the asylums.*—Eight institutions; aggregate of patients 7,516. Between the ages of 20 and 30 years, 2,337; between 30 and 40 years, 1,884. No longer sustaining the result in Dr. Stokes's tables.

4. *Ages at the time of first attack of insanity.*—Nine institutions; aggregate of patients recorded upon this principle, 4,742. Between the ages of 20 and 30 years, 1,712; between 30 and 40 years, 1,106. *The number between* 20 *and* 30 *years exceeds that between* 30 *and* 40, *by a fraction more than* 54 *per cent.* The number of patients forming the basis of this estimate is so large, that the result may be taken as a solution of the question in regard to which decade of life furnishes, absolutely, the most cases of the primary invasion of insanity. But if the decennium of *greatest liability* to the disease be sought, or that in which the greatest *relative* number of cases occur, it can only be found by comparing the number of the occurring cases in each decennial period, with the number of persons of corresponding age in the general population. Dr. Bates has done this for Maine, and Dr. Kirkbride for Pennsylvania, and have shown that, in those States, the *greatest liability* to the disease is between the ages of 20 and 30 years. In the aforementioned article in the *New York Medical Journal*, the cases therefore admitted into the Utica Asylum are compared with the inhabitants of the State, and the result shows that the liability is slightly greater between 30 and 40, than between 20 and 30 years, being as 1000 to 991. We anticipate a different result when the calculation shall be based upon greater numbers, and that in New York, as in other States mentioned, the true period of greatest liability will be found to be in the decennium from 20 to 30 years.

In 1853, twelve patients with suicidal propensity were in the Mt. Hope Asylum; one of homicidal mania, in which the patient killed his wife, and one in which the mother, labouring under puerperal mania, took the life of her infant. "The intellect appeared to be unclouded, and the closest scrutiny could detect no aberration of the mind. The moral feelings appeared benumbed, deadened, perverted.

"General depletion has in no case been resorted to. In many instances it has been found necessary to resort to the local detraction of blood, in order to reduce undue cerebral excitement. * * * * We reiterate the assertion, that if large quantities of blood are abstracted previous to admission, the consequence is that the recovery of the patient is much retarded, if not rendered altogether hopeless. A case of puerperal mania was sent to us, in both of whose arms recent incisions were apparent, and who had been freely cupped on the

neck and temples. The most complete incoherency, after three months' continuance under treatment, exists. In another case of acute mania, in a female aged 65, the lancet had been freely resorted to before admission, without the slightest diminution, but rather with an aggravation of the frenzied excitement. In a few days complete prostration and exhaustion ensued, and she rapidly sank."

Many topics to which we have not alluded are discussed with ability in these reports of Dr. Stokes. We have found it difficult to restrict our extracts within prescribed limits, and have therefore selected only such parts as are of most importance, or the most novel to our readers.

2. Dr. Rockwell, of the Vermont Asylum, never makes voluminous reports. Each of the two now at hand is limited to four pages.

	Men.	Women.	Total.
Patients in the Asylum, August 1, 1851 . .	169	166	335
Admitted in course of the year	79	82	161
Whole number	248	248	496
Discharged, including deaths	73	72	145
Remaining, August 1, 1852	175	176	351
Of those discharged, there were recovered .			78
Died			31

Two of the four pages are occupied by extracts of letters from patients discharged.

	Men.	Women.	Total.
Patients, August 1, 1852	175	176	351
Admitted in course of the year . . .	70	89	159
Whole number	245	265	510
Discharged, including deaths	62	76	138
Remaining, August 1, 1853	183	189	372
Of those discharged, there were recovered .			72
Died			43
Aggregate admitted since the opening of the Asylum			2,066
Recovered			968
Number of deaths not given.			

In March, 1852, variola made its appearance among the inmates of the Asylum. "A short time before the first case occurred, a patient was admitted from a section of country where this disease prevailed, although neither herself nor any of her family had been exposed or suffered from it. We know of no other way in which it could have been introduced. The disease continued with us about ten weeks. One of our nurses and twenty-seven female patients were attacked by it, and only one, a female of seventy-two years of age, died. None of our male patients were affected with this disease."

Two of the wings of the Asylum have recently been enlarged, making additional provision for seventy persons.

3. From the very brief report by Dr. Fonerden, of the Maryland Hospital, we collect the following items:—

	Men.	Women.	Total.
Patients at the beginning of the year 1852 .	63	67	130
Admitted in the course of the year . . .	26	8	34
Whole number	89	75	164
Discharged, including deaths	21	13	34
Remaining, December 31, 1852 . . .	68	62	130
Of those discharged, there were cured . .	5	5	10
Died	4	2	6

Besides the foregoing, seven cases of *mania-à-potu* were received, none of which terminated fatally.

Many applications for admission were refused, and owing to the crowded condition of the wards, it has become necessary to remove the patients belong-

ing to the District of Columbia, a considerable number of whom have heretofore found an asylum at this institution.

It is grateful to learn that measures have been taken towards furnishing the insane of Maryland with accommodations consistent with the present idea of their proper treatment. "The General Assembly, at its session in the year 1852, the first under the new Constitution, passed a law for the erection of a new hospital for the insane; and the commissioners appointed to execute it have purchased a very eligible site, on which they are now proceeding very actively with the work."

4. We have, as usual, two reports from the Lunatic Asylum of South Carolina; one by Dr. Trezevant, the Physician, and the other by Dr. Parker, the Superintendent. The former principally consists of a strong and urgent appeal, in which the inconveniences and defects of the present buildings are described, for a new establishment, located in the country. It is to be hoped that it has not been made in vain.

Patients in the Asylum, November 5, 1851	127
Admitted in course of the year	46
Whole number	173
Discharged, including deaths	38
Remaining, November 5, 1852	135
Of those discharged, there were cured	19
Died, including one suicide	8

From the two reports, we infer that the system of treatment pursued at the institution corresponds, as nearly as circumstances will permit, with that of the hospitals of more recent and more nearly perfect construction.

5. The State Asylum for the Insane, at Jackson, Louisiana, was opened on the 21st of November, 1848. On the morning of that day, eighty-five insane persons were removed from the Charity Hospital in New Orleans, and taken, "in a body" the distance of one hundred and sixty miles, to the Asylum, where they arrived, without accident, on the following day. The statistics, December 31, 1849, as given by the Physician, Dr. Preston Pond, are as follows: "Patients admitted, 130; recovered, 29; discharged, 18; removed, 6; eloped, 7; died, 24." It appears from this, that some are reported as recovered, who have not been discharged.

"The deaths have been, one accidental, two of fits, one of dropsy, one of cholera morbus, and the remainder of chronic diarrhœa."

"The number of deaths is large in proportion to the whole number of patients. The class of persons received here is different from that of almost any other institution of the kind in the United States. A great proportion came from New Orleans, where are congregated a multitude of foreigners from all nations. Many of them are in destitute circumstances, suffer much from improvidence, disappointment, and disease, until their bodily health is destroyed, and when they arrive here life, in many cases, can only be prolonged for a short period."

By the report for 1852 and 1853, it appears that this institution is under the general direction of a president and board of administrators; the executive officers being a physician, a superintendent—who is not a member of the medical profession—and a matron; that it will now accommodate one hundred and forty patients, and when finished will be able to receive two hundred and fifty.

	Men.	Women.	Total.
Patients in the Asylum, Dec. 31, 1851	33	45	78
Admitted in two years, to Dec. 31, 1853	75	48	123
Whole number	108	93	201
Discharged, eloped, and died	40	29	69
Remaining, Dec. 31, 1853	68	64	132
Of those discharged, there were cured	4	9	13
Died	23	17	40

Causes of death.—Chronic diarrhœa 5, diarrhœa 8, phthisis pulmonalis 3, cholera 4, yellow fever 2, marasmus 3, "fits" 3, cholera morbus 3, old age, chlorosis, "disease of the lungs," chronic dysentery, lead colic, dropsy, general palsy, "flux," 1 each; one not mentioned.

The patients who died with yellow fever, and of cholera, had those diseases when admitted. Neither of the epidemics spread among the other inmates of the institution.

"Diarrhœa is the most troublesome disease we have to contend with. It has been treated here with blue mass and Dover's powder, sub-nit. of bismuth, sulph. copper and opium, sulphuric acid, sulphate of iron and sulph. alumen and potassa, acetate of lead and opium, and by cod-liver oil. Some cases have recovered, some have been improved and life prolonged. But generally, when a disposition to chronic diarrhœa is manifested, the disease, though checked, recurs again and again, until it finally terminates in death."

	Men.	Women.	Total.
Whole number of patients admitted . . .	191	134	325
Discharged recovered	30	23	53
Died	60	35	95
6. At the Butler Hospital, the number of patients on the 31st of December, 1852, was . .	64	78	142
Admitted in 1853	45	47	92
Whole number	109	125	234
Discharged, including deaths	46	52	98
Remaining, Dec. 31, 1853	63	73	136
Of those discharged, there were cured . .			44
Died			22

Died of chronic mania 10, "acute maniacal affection" 5, paralysis 2, "meningitis produced by transference of disease" 1, epilepsy 2, uterine disease 1, uncertain 1.

Of twenty-seven discharged improved, it was believed that "many would probably have recovered had they remained a few weeks longer."

Patients admitted since the institution was opened, in 1848 .	583
Discharged recovered	171
Died	102

It will be recollected that in our notice of the last two preceding reports by Dr. Ray, we gave a synopsis of the discussion upon which he entered in regard to the causes of insanity at the present day. The subject is resumed in the report before us, and what is here written, together with that which preceded it, is almost enough to destroy one's confidence in the advantages of civilization.

In resuming the discussion, Dr. Ray informs the reader that mental alienation rarely springs from an individual source, and that consequently, when he treats of a specific cause, it is only as one which occupies a "prominent place in any combination of incidents more or less directly followed by insanity."

Intemperance in the use of alcoholic beverages is first considered; and at the close of this part is the following remark in reference to a certain class of drinkers with whom almost every lunatic asylum is troubled: "Let the legislature enact that habitual drunkenness shall be subjected to all the disabilities of insanity, and then we may engage in a work of humanity without infringing upon the rights of individuals." * * * "Vicious indulgence" furnishes the Hospital at Worcester with about six per cent. of its patients, and to the Butler Hospital a somewhat larger proportion. "There is reason to believe that this form of disease has been increasing with all the contrivances of luxury incident to the progress of civilization. Abundance of stimulating food, allurements of dress, refinement of manners, frequent and intimate social intercourse between the sexes, and, above all, a species of literature especially designed to inflame the passions and fill the mind with seductive images—these are incidents which, whether regarded individually or collectively, have nowhere, and at no time, been so widely prevalent as in this country during the last fifty

years. They constitute the origin of much of the kind of insanity we are considering, and sufficiently explain why it should be more common in American than European communities."

The consumption of vital energy required by the exigencies of civilized life is believed to be a prolific cause of insanity. "There never was a time when brains were more rapidly used up, in one way or another. In the struggle for wealth, power, or distinction, or even for the privilege of living at all, success requires continuous attention, intense application, and a strain of all the faculties to an extent that was once comparatively rare. The disastrous result is usually precipitated by habits of living not calculated to restore the energies thus prodigally expended." Here the author proceeds to condemn in detail the warming of apartments with air heated by red-hot iron, ill-ventilated rooms, neglect of physical exercise, improper food—"bread just from the oven, charged with some deleterious drug and saturated with butter, with meat swimming in gravy, and swallowed with little mastication—not to mention a host of other articles equally improper." The immediate and secondary effects of these agencies are minutely described, and the more remote results, reached by gradation of abnormal phenomena, exhibited.

"No careful observer can help coming to the conclusion that, of all the incidents which tend to develop insanity in the female sex, none is so prolific as what is called 'ill health;' and there can be but little doubt that nowhere is this condition more common than it is with us. This form of disease arises, in a great degree, from excessive domestic labour in conjunction with bad diet, bad air, insufficient recreation, and, in married women, frequent childbearing." Reasons for its greater prevalence here than in similar classes in other countries. "The latter unquestionably work hard and fare hard, but *they start with a stronger constitution; they are much in the open air; they live on plain food, and move in a social sphere that bounds their wishes and aspirations.* Here, the woman enters upon married life with a constitution somewhat delicate. Ambitious that her house and family should be distinguished among her neighbours by all the indications of good management, but unable to indulge in hired service, she labours beyond her strength, and does nothing towards restoring it by suitable relaxation. The cares of an increasing family, without increasing pecuniary means, seems to forbid the slightest rest from the daily routine of toil. The duties are all within doors, in over-heated apartments; while a certain regard for appearances, and a perpetual straining after a higher social sphere, give rise to an uneasy, if not a repining state of mind. At last, the appetite fails, less and less food is taken into the stomach, the nervous system becomes irritable under the slightest impression, the sleep is diminished, the flesh reduced, and the mind is depressed by unaccountable gloom and apprehension. The end is now at hand in the shape of unequivocal insanity, from which recovery is tedious, at best, and often hopeless."

The subject of hereditary transmission is next discussed, but without adducing any new feature or effect of importance. Its operation in increasing the prevalence of insanity is thus alluded to: "I think we are warranted in saying that insanity in persons who subsequently become parents, is, upon the average, duplicated in the next generation. We must, therefore, shut our eyes to this trait of insanity—its self-multiplying power—before we can doubt the fact of its rapid increase. A larger knowledge of the organic laws, and of the penalties that follow their violation, will, undoubtedly, check the evil, but this power will too often be overborne by the other agencies that are concerned in the case. When reason, prudence, foresight, inculcate one lesson; and passion, sentiment, and taste another, it is obvious enough which will generally prevail.

"The operation of the physical causes here mentioned is aided, in this country, there is much reason to believe, by peculiar atmospheric conditions. * * * That our people are distinguished by restlessness, impulsiveness, impetuous and boisterous movement, may be regarded as a fixed fact. That this trait is to be attributed to atmospherical influences, is rendered probable both by the absence of any other adequate cause, and by the greater excitability which accompanies insanity in this country, as compared with others;" and "this excitability is but little less apparent in the Germans, Irish, and English, who abound in our hospitals, than in the native Americans." P. E.

ART. XVII.—*On the Structure and Use of the Spleen.* By HENRY GRAY, F. R. S., Demonstrator of Anatomy, and Surgical Curator of the Pathological Museum of St. George's Hospital. London, 1854. 8vo. pp. 380.

THIS admirable work, for which the physicians and surgeons of Guy's Hospital awarded to its author the munificent prize endowed by the late Sir Astley Cooper, is an honour to British science. For elaborate research and experiment, it is not excelled by the famous labours of Lyonnet in the anatomy of the *Cossus ligniperda*. The investigations of previous writers have given us correct notions of the anatomical structure of the spleen—and, indeed, most of the results of the researches of Mr. Gray and his coadjutors; but none have so systematically, patiently, and accurately unravelled its structure and functions, as the latter.

After an historical introduction, the development of the spleen and its various tissues are first considered; and results of the author's observations clearly prove that the organ does not attain its maximum of development during fœtal life, and that its conditions of utility need not be sought for during that period. He further adds: "If it be admitted that the spleen, like other organs, is performing its function to the fullest possible extent when it attains its largest size, and when it arrives at its maximum of development and growth, then it is proper that we should observe at what period this occurs." A table is then presented of one hundred and sixty observations by which, and others of a similar character, the author arrives at the following conclusions:—

"1. That the size, and consequently the activity of the spleen, are called into action from the period of birth to the termination of adult life.

"2. That its size, and consequently its function, are periodically manifested, and increased during and after the completion of the digestive process.

"3. Its size varies considerably according to the state of nutrition of the body, being increased in highly fed, and being diminished in starved animals.

"4. Independent of these extremes of size, consequent upon the nutrition of the body, it increases considerably after the completion of the digestive process in the highly fed animals; whilst in the opposite case, no difference in its size is observed."

The structure of the spleen is next minutely investigated, the results of which very generally accord with the researches of Kolliker, Ecker, Gerlach, &c.

In the chapter on the bloodvessels of the spleen, the author confirms the fact that each of the main trunks of the splenic artery supplies simply that region of the organ in which that branch ramifies, no anastomoses existing between it and the majority of the other branches.[1] The capillaries terminate by becoming continuous with the smaller venous trunks; but, as previously mentioned by Ecker, some of them cannot be traced directly continuous with the veins, and these communicate with interspaces of the pulp parenchyma which are finally connected with the veins.

From a number of experiments on horses it is shown that the quantity of splenic blood varies under different circumstances. The greatest amount exists in the organ just after the completion of digestion, and the new material has been received into the circulation. The smallest amount is found long after the digestive process has ceased, and the new material has been expended in nutrition, secretion, &c. The quantity also varies with the character of the nutrition of the animals; in ill-fed animals being small, and in those which have been starved, it is reduced to its minimum. The introduction of fluids, also, especially such as are quickly absorbed, considerably influences the increase of the blood; and, of course, while the direct transfusion of blood into the circulation increases that of the spleen, its withdrawal in a corresponding degree decreases it.

[1] Several preparations by Dr. Horner, in the Museum of the University of Pennsylvania, are beautifully illustrative of this fact.

The spleen is likewise found to act as a reservoir for blood when the circulation is obstructed as a consequence of impeded respiration. Experiment alone does not exhibit this result, for it is observed that diseases of certain organs, as the heart and liver, attended with obstruction to the circulation, are in all cases accompanied with enlargement of the spleen, and distension of its vessels.

From numerous analyses it was ascertained that the emerging blood of the spleen, as compared with the arterial blood entering the organ, and with the blood of the general venous and portal systems, presents the following peculiarities. It contains less solid matter than arterial or other venous blood, far less blood-globules, a considerable increase in the amount of albumen and fibrin, more fat, a variable amount of iron; and lastly, its serum presents in all cases a dark-reddish tinge.

An attentive study of the splenic pulp confirms the view of Kolliker, that the blood-corpuscles undergo disintegration in the spleen; and it is to this cause we must attribute the peculiarities of the splenic venous blood when compared with that which enters the organ.

After investigating the structure of the Malpighian corpuscles, and determining the laws which regulate their variation in size, the author considers that his results afford sufficient evidence of the glandular nature of these bodies, and of their close similarity in structure and function with the corpuscles of other ductless glands.

The last chapter of the work is on the physiology of the spleen; and in this the author enters into a consideration of the facts resulting from his labours, and concludes that the function of the spleen is to regulate the quantity and quality of the blood. J. L.

ART. XVIII.—*Epilepsy and other Affections of the Nervous System which are marked by Tremor, Convulsion, or Spasm—their Pathology and Treatment.* By CHARLES BLAND RADCLIFFE, M. D., Licentiate of the Royal College of Physicians, Assistant Physician to the Westminster Hospital, Lecturer on Materia Medica and Therapeutics at the Westminster Hospital School of Medicine, etc. etc. London, 1854. 8vo. pp. 144.

THOSE of our readers who have had an opportunity of perusing Dr. Radcliffe's treatise on *The Philosophy of Vital Motion*, are aware that he has advanced a new and somewhat startling theory of muscular motion. That muscular contraction is a purely physical phenomenon dependent on ordinary molecular attraction when the muscle is *not* stimulated. That the real operation of nervous and other vital agencies, and of electricity and other physical forces, is not, as usually taught, to excite or stimulate contraction in muscle and other organic tissues, but to counteract this state, and induce relaxation or expansion. In other words, that all stimulants, vital and physical, so far from producing, on the contrary antagonize, muscular contraction, which occurs only upon the suspension or withdrawal of the action of the stimulants, in a manner perfectly analogous to that which takes place in a bar of metal when heat is withdrawn.

This physiological heresy, an exposition of which prefaces the proper subject of the essay before us, is enforced and illustrated by a series of arguments and illustrations of a most acute and plausible character, which cannot but arrest attention, even though they fail to carry conviction of the truth of Dr. Radcliffe's theory of vital motion.

Upon that theory is based, in the volume before us, the pathology and treatment of epilepsy, and other nervous affections marked by tremor, convulsion and spasm, as, the tremors of delicate and aged persons, chorea, paralysis agitans, delirium tremens, the rigor and subsultus of fever, the tremor of mercurial poisoning, the convulsions of fever, of hydrophobia, of saturnine and hydrocyanic acid poisoning, and those dependent upon the retention of urea

in the blood; the convulsions of cerebral disorder, of uterine disorder, and of intestinal disorder, and the convulsions of death; laryngeal spasm, cholera cramps, tetanus, ergotism, catalepsy, and rigor mortis.

Whatever estimate may be formed of Dr. Radcliffe's views of the nature and cause of muscular contraction and relaxation, and the correctness of the application of these views to explain the pathology of epilepsy and cognate affections, there can be little doubt that his theory of these diseases has a surer basis in truth—in many, at least, of its features—than that commonly received.

On an analysis of the leading phenomena of epilepsy, Dr. R. shows that neither the vascular, nervous, nor muscular systems present any indications of morbid excitation, but the reverse.

The condition of the skin, the state of the pulse, and the general diminution of the temperature of the surface, but especially of the extremities, indicate a very decided depression of the circulation. During the height of the fit, the state of the circulation is one of prostration, verging upon actual extinction.

The entire condition of the epileptic indicates the same want of activity in the nervous as in the vascular system. "The intellectual and memorial faculties fail day by day under the blight which eventually obliterates them. On the eve of the fit, the patient is rarely otherwise than silent, sad, moody, and still; in the fit, he is bereft of all sensibility, consciousness, and volition, and for sometime afterwards he is stupid, confused, and exhausted."

"Arguing from the state of the pulse and the respiration in epilepsy, it would appear," Dr. R. remarks, "that the *medulla oblongata*, the *spinal cord*, and the *sympathetic ganglia* are in the same condition of inactivity as the brain. Nay, it cannot be otherwise, for the functional energy of these organs, as of the system generally, must be in direct relation to the activity of the circulatory and respiratory changes. It is to be observed, also, that that remarkable want of tone, which is a very marked peculiarity of the epileptic, is indicative of the want of energy in the spinal cord, if it be a function of this organ to supply this tone."

The depressed condition of the muscular system is evinced in the epileptic by the remarkable inadequacy to exertion; in the slowness with which the system rallies after fatigue, as well as in the pallid and soft condition of the muscles on dissection, which condition contrasts very strongly with the normal redness and consistency of these organs.

"Viewed in this manner," says Dr. R., "the vascular and nervous systems of the epileptic, as well as the mobile structures in which the convulsive phenomena are manifested, are seen to present unequivocal evidences of inactivity, and this inactivity—so far at least as the vascular and nervous systems are concerned—is found to be most marked in the fit itself."

The causes, or supposed causes, which operate in the induction of epilepsy are, according to Dr. R., all of a depressing nature.

"Thus," he remarks, "the seizure is referable, not to joy, but to fright and fear—not to any natural excitement, but to the exhaustion consequent upon excess and abuse—not to good cheer, but to hunger and privation. It happens at night, when the vivifying influence of the sun is withdrawn, rather than the day. Much obscurity hangs over these matters, from the careless manner in which the most incongruous agencies have been grouped together as exercising the same influence upon the system, and much obscurity is inevitable from the difficulty of untying the complex knot which holds together the several influences acting upon the body; but there is no reason to doubt, and every reason to believe, that these several causes of the fit are of an exhausting and not of an exciting character."

On a review of the foregoing facts and considerations, Dr. R. considers it to be sufficiently evident that—

"Epilepsy cannot be caused by any excitement of the muscles consequent upon the excessive supply of nervous or any other stimulus. On the contrary, everything is in harmony with the physiological premises, and, as might be anticipated from these premises, the convulsion would seem to depend upon *want* of vital stimulation, which want had allowed the molecular attraction of the muscles to come into play and gain the ascendency."

Although we are not prepared to give our assent to the position which Dr. R. attempts to establish, namely, that all stimulants, vital and physical, in place of exciting or promoting, on the contrary antagonize muscular contraction, we nevertheless admit the correctness of his views in relation to the condition of the circulatory, nervous, and muscular systems in the epileptic—everything connected with the phenomena and progress of his morbid condition, evinces a diminution of vigor—of vital energy—a depression of their normal functions; each of these systems is in a condition of *under*, and not of *over activity*.

Dr. R. endeavours to show that the same depressed activity in the vital functions obtains in all the affections allied to epilepsy—those, namely, of which tremor, convulsion, or spasm constitute a prominent system. The evidence of depressed vital energy are in many of these affections sufficiently apparent, and even in those where the evidence is of a less striking character, it will not be found wanting, upon a careful analysis of the circumstances under which the convulsive or spasmodic phenomena present themselves, and the condition of the circulatory, nervous, and muscular systems preceding and attending them.

"The subjects of nervous trembling," Dr. R. remarks, "have a certain delicacy of constitution which cannot be overlooked, and if not women, they have the feminine habit of body in a very marked degree. Those who tremble from old age or from shaking palsy, present unequivocal marks of decrepitude and decay—the listless wish, the snowy or hairless head, the fireless countenance, the wasted limb, the feeble pulse. Chorea is almost peculiar to females, and to females whose parents were infirm or aged, or who themselves have become enfeebled by improper or injudicious habits. It often originates during some severe and exhausting disease, and is always accompanied by signs of debility. The atony of the circulation is usually indicated by paleness of the lips, face, and tongue, by pastiness of the skin, by effusion into the serous cavities, and by rhéumatic deposits."

Convulsion happens most frequently in women and children, and but rarely in men, and this fact is an argument that other convulsive disorders besides epilepsy are connected with a state which is characterized by weakness rather than by strength.

"Spasm, again, occurs more frequently in women than in men, and most frequently in the more irritable and weakly of women. Cramp is the constant companion of tremulousness. It increases with the advances of age, and is almost permanent when nervous tremors become intensified into palsied tremors. The subjects of catalepsy, in like manner, are more delicate and impressible than they ought to be. Their skin is usually pale or dingy, their pulse readily disturbed, their general appearance either hysterical or apathetic. A cataleptic boy, a patient of my own, was as irritable, uncertain, and fretful as an infant. His apprehension was slow, his memory weak, his head large, his eyes staring, his pupils dilated and sluggish, his complexion sallow and venous, his hand cold and clammy, his pulse slow and feeble; and lastly, the state in *rigor mortis* is expressed by the name."

The consideration of the pathological condition of the vascular, nervous, and muscular systems in the several diseases and stages of disease marked by tremors, convulsions, and spasm, is followed by a chapter on periodicity. The views of the author in reference to this interesting subject are far from satisfactory, and cannot be received in their full extent as explanatory of the periodicity of disease until established by more convincing facts than have as yet been adduced in their support.

After noticing the indications of periodicity in the physiological phenomena of plants and the lower order of animals, Dr. R. observes:—

"In spite of every cause of perturbation, there are many evidences of periodicity even in man. It is something more than accident which so often causes man to be stunted, stolid, and passionless in countries where cold reigns without a rival. It is something more than accident which times the periodical changes of women by the lunar orbit. There are, indeed, frequent irregularities in all these cases, and particularly in the last, and these may be supposed to be the natural results of the changing periods of food and passion, but the law is apparent above every irregularity.

"It would appear, then, that there are certain periodical changes in vital phenomena which reflect more or less distinctly the movements of the sun and moon, some of them corresponding to the day, others to the month, and others to the year; and that these changes are more and more conspicuous the lower the grade of organization in which they are displayed—more so in woman than in man, more in animals at the foot of the scale of being than those at the summit, and most of all in the plant.

"Returning now to the bedside, it may be expected that the signs of periodicity will always be masked and obscure in man, but that they will be manifested most distinctly in him who is most deprived of that active inherent life which constitutes the badge of distinction between man and the plant, and not in the person who is acted upon by inflammation, or who is excited in any other way. And so it is.

"There can be no doubt as to the obscurity of the evidences of periodicity even where that obscurity is least, as in epilepsy and in affections allied to epilepsy; but there can also be no doubt as to the existence of these evidences. Thus, on looking at a number of cases, it is found that convulsion and spasm occur more frequently at night than in the day, more frequently about the time of new moon, than about the time of full moon, and more frequently in the winter months than in the summer months. Of these evidences of diurnal, monthly, and annual periodicity, the diurnal are the most frequent, and the best established; but all are sufficiently frequent and obvious to convince any one who will take the trouble to seek after them for himself, or to consult the admirable little treatise of Dr. Mead: *De imperio solis ac Lunæ in corpora Humana et morbis inde oriundis.* They are not, perhaps, sufficiently frequent and obvious to allow any theory to be based upon them, but it is impossible to omit noticing that the greater frequency of convulsion and spasm in the night, at the times of the new moon, and during the winter months, is in accordance with the preceding pathological doctrines, and that, being thus in accordance, it is an additional confirmation of the correctness of those doctrines.

"It appears, therefore, that the signs of morbid periodicity are manifested most distinctly in the person who is most deprived of that inherent life which constitutes the badge of distinction between man and the plant, and not in the person who is acted upon by inflammation, or who is excited in any other way; and being so they furnish an important confirmation of doctrine, and a new rule of treatment. The confirmation of doctrine is obvious, for in this point of view the signs of periodicity become only so many additional evidences of that constitutional want of innate strength which appears to be the prominent fact in the pathology of epilepsy and of the cognate disorders. A new plan of treatment is equally obvious, for if the signs of periodicity depend upon a simple want of innate strength, then it becomes necessary to abandon all those leeching and starving plans of treatment which have originated in the supposition that they depended upon internal inflammation, or fever, or some other state of excitement, and to adopt in their stead all the means which are calculated to arouse and invigorate the downcast and flagging powers of the system."

The general plan of treatment for epilepsy and the affections allied to it, laid down by Dr. R., is unquestionably the correct one. At the same time we think he depends too exclusively upon a tonic and stimulating course, to the exclusion of all other remedial measures, however strongly they may seem to be indicated by the morbid condition of various organs which is so often associated with epilepsy and the cognate diseases.

A full diet of nutritive and easily digestible animal food, without the usual quantities of farinaceous and green vegetable matters, is insisted upon by Dr. R., as essential in all cases of epilepsy.

The propriety of such a diet in all cases in which the patient can be induced to take it is admitted. In many cases, however, the appetite and digestive powers of the stomach are so impaired in the epileptic, that the patient is either averse to taking food of a proper kind, or if he can be induced to do so even to a very moderate extent, it produces considerable distress and other unpleasant symptoms. In such cases, the digestive powers of the stomach must be restored by an appropriate course of treatment before the full animal diet, recommended

by Dr. R., can be carried into effect, with any hopes of deriving from it the desired restorative and invigorating effects.

Dr. R. is in the habit of recommending a very liberal allowance of alcoholic stimulants to his epileptic patients; and he states that, from the result of this practice, he is fully satisfied that there is no disease in which they are more needed. He believes epileptics to be benefited by the use of coffee, and injured by the use of tea.

That there may occur many cases of epilepsy, in which the use of alcoholic stimulants are required, we admit; but we fear that, to allow their "very liberal use" in all cases, indiscriminately, would be apt to do far more injury than good. We can see no objection to the substitution of coffee for tea.

The epileptic's habits should be so ordered as to save the strength as much as possible. Hence Dr. R. very properly advises celibacy, abstinence from any severe study, and from bodily exercise.

"Often," Dr. R. remarks, "I have found a patient to improve in a marked and unmistakable manner as soon as he had had the resolution to conquer the fidgetiness which is invariably connected with debility, and to force himself to rest; and often I have known a patient begin to retrograde if he had begun to try his strength too soon. Only the other day, I had a note from a medical gentleman, in which he told me that a patient, about whom he had consulted me, had gone on very well so long as he had made a point of riding to his place of business, and that the fits had returned as soon as he had begun to disregard this direction."

As tonics in epilepsy, Dr. R. prefers quinia and iron.

Of turpentine he speaks in the highest praise, as "the remedy which stands foremost among those which have rendered unequivocal service in epilepsy." Its nauseous taste, however, and the irritation which it excites in the urinary and generative organs, have always been a serious objection to it, and the result has been that comparatively few patients have the resolution to persevere in its use as long as is necessary to insure permanent benefit. Dr. R. was, in consequence, induced to substitute naphtha for turpentine, and found that in doses of half a drachm to a drachm it produced the same decided relief as the turpentine; but it was scarcely less disagreeable, and patients could not be induced to take it for any length of time.

"After this," says Dr. R., "it occurred to me to try camphor, and this I did in doses of two or three grains, either alone or in combination with quinia or iron, one or both, according to circumstances. Being given in the form of pills, it was free from the principal objection applying to the two former stimulants, and it had this peculiar advantage, that instead of irritating the urinary and generative organs like turpentine, it exercised, or seemed to exercise, a direct quieting influence upon them. In other respects, as tried in several cases, the result was not less satisfactory.

"Next in order of time I gave a fair trial to chloric ether, and still with very decided benefit. Under ordinary circumstances, I gave half-drachm doses of this preparation, either alone or in combination with the ammonio-citrate of iron, or quinia, or naphtha, and in all cases it proved to be a very favourite and effective remedy, particularly with children. Sometimes I substituted Hoffman's anodyne in place of the chloric ether; and sometimes, when the need of a stimulant has seemed to be very urgent, I have associated the two, but it has always seemed that this form of ether is far less efficacious than chloric ether.

"In cases where, and at times when, an occasional stimulant effect was necessary, I have recommended the aromatic spirit of ammonia, either alone or in combination with ether, and the result has usually been certain and satisfactory."

In addition to internal stimulants, Dr. R. employs, externally, counter-irritants, and the hot bath.

Dr. R. recommends a hot bath daily, and an additional one whenever any unusual depression tends to the apprehension of a fit. From the adoption of this practice he has seen many instances of marked and decided benefit.

"This plan," he remarks, "seems to be equally desirable in long-standing cases, where there is much cerebral congestion, and theoretically and practi-

cally there is much reason to believe that this is what might be expected, especially if a towel dipped in cold water be wrapped around the head of the patient while he remains in the hot bath.

"In actual practice," says Dr. R., "I have rung changes upon these different stimulants, either giving them alone or combining them with iron and quinia, substituting one for another according to the changing circumstances of the case, and always allowing at the same time a liberal supply of dietetic stimulants, upon which, indeed, hope is mainly to be based, and I have every reason to be satisfied with the results. I have never met with a patient who has not been benefited; for even where the case has been of long standing, and the fits have kept their ground, there has been a manifest diminution of intellectual torpor, the face has lost a good deal of the brutalized expression which had been creeping over it, and the distressing nervous headache has disappeared, if that symptom had been present—and I have met with many patients who have been completely cured.

"If there is one time more than another when stimulants are necessary, it is on the eve of the fit. Then, vigorously administered, they will often prevent the paroxysm. Nor are they contra-indicated in the fit itself. At this time all that is usually required is to raise the head as much as possible, so that the blood may not gravitate into it, and to unloose the neckerchief and shirt-band; but if more is required, it is still upon stimulants that dependence must be placed, and this equally, whether the circulation be in a syncopal or asphyxial state. Indeed, under these circumstances, the proper course is to dip a door-key or hammer-head into boiling water, and apply it to the pit of the stomach, or to put the patient into a hot bath, or to take advantage of a moment of quiet and inject a turpentine enema into the rectum. Nor are stimulants contra-indicated after the fit, except perhaps during the first few moments of the reaction which follows upon the collapse, and this only in some instances, for often this reaction is not up to the normal standard. Nay, they are not necessarily contraindicated by the mental excitement which occasionally supervenes upon the fit, for this excitement is usually, if not always, of an asthenic character.

"All these considerations are in harmony with what might be gathered from a simple inspection of the state of the pulse (the true key to practice), and the conclusion is that stimulants will be found to be the proper remedies for epilepsy, if they are given with discrimination, and regulated in quantity according to the heat or coldness of the season."

With regard to tracheotomy as a remedy in epilepsy, Dr. R. remarks that it is not easy to come to a decision, inasmuch, especially, as there is an insufficiency of evidence on the subject.

"Still," he observes, "it is clear that it does not fulfil all the original expectations of Dr. Marshall Hall concerning it. It does not prevent convulsion. It does not always, or even usually, make the convulsions lighter. It does not prevent danger, for of the few patients upon whom the operation has been performed, three have died either in the fit or in connection with the fit, and of these three, the opening in the windpipe was free from obstruction—at least in one. Under these circumstances, it becomes a question whether the benefits of the operation are sufficient to counterbalance the associated inconveniences and dangers, even where (which rarely happens) the asphyxial symptoms are consequent upon spasmodic closure of the larynx—and this question must remain in abeyance for the present.

"——— As to the rest, it only remains to be said that the accustomed rules of treatment must be applied to the correction of any special source of exhaustion, and particularly of those which are peculiar to female epileptics."

In reference to the treatment of affections allied to epilepsy, Dr. R. recommends a similar invigorating, tonic, and stimulating course. The general conclusion is—

"That epilepsy and the cognate disorders must be treated upon the same principles, and that upon these principles every cause of depression and exhaustion must be avoided, every means of increasing and establishing the strength must be sought after, and stimulants must be trusted to as the grand

agents in recovery. In a word, physiology, pathology, and therapeutics concur in showing the necessity of a complete revolution in everything relating to the theory and practice of the maladies which have been under consideration, and they also justify the hope that in future the theory will not be a subject of mystery, or the practice a source of conjecture, perplexity, and failure."

The essay of Dr. Radcliffe is well deserving of an attentive perusal on the part of the medical practitioner. The views of the author in reference to the nature of muscular contraction, and the influence exercised upon it by nervous and other vital agencies and by physical forces may, it is true, receive but little favour; nevertheless, his general conclusions as to the pathological conditions under which tremor, convulsion, and spasm ordinarily occur, which are by no means necessarily dependent upon the truth of his physiological doctrines, as well as his general directions for the treatment of those affections of which tremor, convulsion or spasm is the prominent characteristic, are worthy of serious consideration.

D. F. C.

ART. XIX.—*A Clinical Introduction to the Practice of Auscultation, and other Modes of Physical Diagnosis, in Diseases of the Lungs and Heart.* By H. M. HUGHES, M. D., Fellow of the Royal College of Physicians, &c. Second American from the second and revised English edition. Philadelphia, Blanchard & Lea, 1854. 12mo. pp. 304.

THIS little work is of that kind which will surely meet with many readers. The author has contrived to present to the student, in a comparatively small compass, a comprehensive, accurate, and well-arranged exposition of the leading principles and facts in the physical diagnosis of diseases of the chest. His language is plain, and the book is free from all that unnecessary discussion of the laws of sound, and of doubtful questions which are so apt to embarrass the learner in his efforts to discern and comprehend the essential points in this important branch of medicine. As its merits consist mainly in the manner in which the subject is presented, we shall be excused from offering an analysis of the work; it is only necessary to state that the author expresses his dissent from the views of Skoda, especially in the very general application of the "*laws of consonance*" to diseases of the chest. We append, however, the following explanation of a curious fact: "In some cases, which are not so rare as it has been by some authors supposed, it happens that though the pleura is actually full of fluid, and may be proved to be so by inspection after death, not only is tubular breathing general, but a shrill resonance of the voice is distinctly audible over the whole side. Though the lung be compressed into the smallest space, and the bronchial tubes be therefore far removed from the solid parietes, in all parts excepting in the neighbourhood of the spinal column, still, their vibrations are communicated to those parietes, and from them to the ear. How does this happen? It is, indeed, difficult positively to assert how; and because difficult to explain, the fact itself may be questioned. The same thing, however, in regard to the voice, as well as to the tubular breathing, has occurred in persons from whose chests, immediately after the examination and observation of these signs, several pints of fluid have been drawn off by paracentesis. How, then, are these circumstances to be explained? It is acknowledgedly one of those cases in which ordinary acoustic principles appear (most probably from our not being aware of the whole or the exact circumstances of the case) to be insufficient for the satisfactory explanation of what is occasionally heard in the practice of physical diagnosis. If a conjecture might be ventured upon the subject, it would be this: so long as there exists any portion of the lung which is capable of further compression; so long, therefore, as the parietes are not tense, and the fluid contained in them, although in large quantity, is movable, so long is bronchial respiration inaudible over the general surface of the side. But when the compression is great enough to render the

parietes and the fluid contained within them tense, but not so great as to flatten or absolutely to close the larger bronchial tubes, then the vibration of those tubes is communicated to the spine, or to the bony parietes, against which they rest, and thence, as in a well-braced drum, to the walls of the entire cavity. The fluid is so tightly pressed that it moves as a solid, and conveys the vibrations as a solid to, and with the solid walls. * * * In the former edition of this work, I spoke doubtfully and hesitatingly in reference to this curious subject, and as if it were of very rare occurrence—as it has, indeed, been represented by most authors—seeing that my attention had been directed to it only in a few instances. From many cases, however, which I have since examined previously to the operation of *paracentesis thoracis*, and from many others in which almost equal evidence existed of the presence and accumulation of fluid in the pleura, I am quite certain that it is not uncommon; I feel assured that it exists as the rule; and I very much question whether, if some accidental circumstance does not exist to prevent its development, it is not generally present when one side of the chest is fully distended with fluid."

For soundness of doctrine, and practical good sense, there are few manuals of physical diagnosis which can compare with this; it therefore recommends itself to the attention of all who desire to possess accurate knowledge on this subject.

M. S.

Art. XX.—*Auscultation and Percussion.* By Dr. Joseph Skoda. Translated from the fourth edition by W. O. Markham, M. D., Assistant Physician to St. Mary's Hospital. Philadelphia, Lindsay & Blakiston, 1854. 12mo. pp. 380.

To those who are not familiar with the modifications which Prof. Skoda has endeavoured to make in the theory and nomenclature of the sounds heard in the lungs and heart, this translation of the last edition of the Viennese professor's work will be highly acceptable. The following may be taken as a hasty sketch of his peculiar views:—

Rejecting the pectoriloquy and œgophony of Laennec as being merely modifications of the thoracic voice, uncertain in their diagnostic value, he recognizes only the following:—

1. The voice, accompanied by a concussion in the ear, completely traverses the stethoscope—loud bronchophony, which may be either clear or dull.
2. The voice, unaccompanied by concussion in the ear, passes incompletely through the stethoscope—weak bronchophony.
3. An indistinct humming (*sümmen*), with or without a barely appreciable concussion in the ear.
4. Amphoric resonance, and the metallic echo of the voice. Considering, moreover, Laennec's bronchial and cavernous respiration to be identical, and that there are respiratory murmurs to be heard over the chest, which cannot be classed under the head either of bronchial or vesicular murmurs, he institutes the following division:—
1. The pulmonary respiratory murmur, or vesicular breathing of Andral.
2. Bronchial breathing.
3. Amphoric echo and metallic tinkling heard during breathing.
4. Indeterminate (*unbestimmte*) respiratory murmurs.

Furthermore, he divides the râles as he does the voice and respiration, "so far only as the division has a practical value."

1. Vesicular râle.
2. Consonating râle.
3. Dry crepitating râle, with large bubbles, or *craquement.*
4. Indeterminate râles.
5. Râles accompanied by an amphoric resonance and metallic tinkling.

From the above, it will be seen that Dr. Skoda aims not only at a simplification of the nomenclature of Laennec, but denies the accuracy of the facts upon

which it is based. Some of the fundamental doctrines which Laennec proposed, to account for the transmission of sounds within the chest, and upon which rests the diagnosis of the most frequent diseases of the lung, are completely revolutionized by our author's system. With many acknowledged imperfections, but with daily universal testimony to its general accuracy, as well as its occasional defects, Laennec's immortal work is still destined, we believe, to occupy the same place it has hitherto held. The deceptive character of the experiments upon which Skoda's system is founded, and the many discrepancies which are palpable in its application, deprive his innovations of that success which the wonderful diagnostic skill of their author would gain for them. In order, however, that the reader may acquire a correct idea of the new theory of the production and transmission of sound in the lung, we transcribe the following summary of it from Dr. Markham's preface:—

"* * * The voice passes into the parenchyma of the lungs through the medium of the air in the trachea and the bronchial tubes, and is not propagated along their walls; it traverses healthy as readily as it does hepatized lung, and even somewhat more readily; consequently, bronchophony does not depend upon an increase of the sound-conducting power of consolidated pulmonary tissue; moreover, when the lung is consolidated, the thoracic voice increases and diminishes in force, without any concurrent change taking place in the condition of the lung. This variation in its strength evidently results from the circumstance of the bronchial tubes being at one moment blocked up by mucus, &c., and at another freed therefrom by the cough and expectoration, &c. If the bronchophony depended upon the conduction of sound, it would be a matter of indifference whether the tubes contained air or fluids. It must not be forgotten that, according to the ordinary laws of reflection of sound, the more solid the parenchyma, the more difficult does the passage of the sound from the air into it become. That the air in the mouth and nasal cavities consonates with sound formed in the larynx, is proved by the fact of the changes which the voice undergoes through opening and closing the mouth and nose, whilst the condition of the larynx remains unaltered; just in the same way does the air in the trachea and bronchial tubes consonate with the laryngeal sound. Now, air consonates only in a confined space, and the force of the consonance depends upon the form and size of the space, and upon the nature of the walls forming it; the more solid the walls, the more completely will the sound be reflected, and the more forcible the consonance. The cause of the loud voice produced by a speaking-trumpet is well known. But the air will consonate with certain sounds only; in the trachea and bronchial tubes, it becomes consonant with the laryngeal voice, in so far as their walls have a like or analogous character to the walls of the larynx, of the mouth, and of the nose. Within the cartilaginous walls of the trachea and the bronchial trunks, the voice consonates nearly as forcibly as in the larynx; but as the bronchial tubes divide in the lungs, they lose their cartilaginous character, becoming at last merely membranous in structure, and therefore very ill adapted for consonance; when, therefore, the consonance is increased in these latter tubes, we may be sure, either that the membrane forming them has become very dense or cartilaginous, or that the tissue around them is condensed and deprived of air, whereby the sound-reflecting power of the tubes is increased. Of course, the communication between the air in the tubes and the air in the larynx must be uninterrupted. The walls of a confined space frequently vibrate in unison with sounds excited within it, as do those of an organ-pipe or of a speaking-trumpet. The larynx vibrates with every sound, and its vibrations are perceptible at a considerable distance from their point of origin; so, also, must the walls of the bronchial tubes, which are distributed through the parenchyma of the lungs, vibrate when the voice consonates within them; and the vibrations thus excited will extend to the surface of the thorax, passing through several inches of thick fleshy parts, or of fluids, and manifest themselves there as the consonating sounds of the bronchial tubes."

The author explains the respiratory murmurs and râles upon the same principle. His account of the cause of the impulse, sounds, and murmurs of the heart offers much that is interesting. The second part of the treatise is, however, that which will command the most attention, since in it are presented the

results of close and long-continued clinical observation of the phenomena elicited by physical diagnosis. Even without assenting to the correctness of the author's explanation of acoustic phenomena, or approving of his nomenclature, the reader can still not fail to derive much instruction from consulting this portion of the work. The translation is an excellent one, and its republication will, no doubt, be acceptable to many. M. S.

Art. XXI.—*The Pathology and Treatment of Pulmonary Tuberculosis, and on the Local Medication of Pharyngeal and Laryngeal Diseases frequently mistaken for, or associated with Phthisis.* By John Hughes Bennett, M. D., F. R. S. E., Professor of the Institutes of Medicine in the University of Edinburgh, etc. etc. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 130.

Most of the readers of this *Journal* are familiar with this work, from its having been already issued with the *Medical News* during the first half of the year. To them we need not point out its value, and on those who have not yet studied the important facts contained in it, we would respectfully urge its perusal. In addition to a most lucid and rational account of the pathology of the disease, the author furnishes indications for treatment (illustrated by cases of his own), which are worthy not only of close study, but of general imitation. His labours are directed to the end of impressing upon the mind the fact that the treatment of tuberculosis should be a curative, and not merely a palliative one. His own success warrants him in this recommendation, and in this case, at least, there can be no doubt of the entire accuracy of diagnosis and trustworthiness of detail. M. S.

Art. XXII.—*Clinical Lectures on Pulmonary Consumption.* By Theophilus Thompson, M. D., F. R. S., Fellow of the Royal College of Physicians, London; Physician to the Hospital for Consumption and Diseases of the Chest, etc. Philadelphia, Lindsay & Blakiston, 1854. 8vo. pp. 259.

These lectures were originally delivered at the Brompton Hospital for Consumption, during the spring of 1851, and were published in the *Lancet* of that year. Without being at all systematic in their arrangement, they will be found to contain a good deal of valuable information relative to the statistics and treatment of consumption. The experience of the author with the cod-liver oil is highly favourable, and he has also found some of the animal and vegetable oils of great service. In some cases, indeed, the neat's-foot oil was used with signal advantage after the cod-liver oil had failed to effect any good. Dr. Thomson has been led to rely with a good deal of confidence upon a mark at the reflected edges of the gums of a deeper colour than the adjoining surface—as strongly indicative of a consumptive diathesis. He gives the following results as applicable to diagnosis:—

"1. That the absence of the streak in men affected with inconclusive symptoms of consumption, may incline you to a favourable interpretation of any such suspicious indications, but that in women rather less weight is to be attributed to this negative sign.

"2. That the presence of the sign in women is almost conclusive evidence of the existence of the tubercular element in the blood. When in either sex it coincides with a pulse not materially altered in frequency, by change from the sitting to the standing posture, the presence of phthisis may with high probability be assumed, even before having recourse to auscultation."

With this brief notice, we beg leave to refer the reader to the "Lectures" for an explanation of this phenomenon, and for many valuable suggestions relative to the treatment of the disease. M. S.

ART. XXIII.—*On Gout; its History, its Causes, and its Cure.* By WILLIAM GAIRDNER, M. D. Third edition. London, 1854. 12mo. pp. 400.

WE are pleased to find that the very favourable estimate we formed of this treatise on its first appearance, has been sustained by the suffrage of the medical profession generally, as is shown by the demand for a third edition within the short period that has elapsed since the first was issued.

This work is unquestionably a highly interesting and instructive one, and deserving of an attentive perusal on the part of every physician whose sphere of practice renders him liable to be repeatedly consulted by subjects predisposed to, or by patients already labouring under the attack of gout in its regular or irregular forms, or in those cases where its strange and confusing association with other known forms of disease, cause a decided change in the character and progress of the latter, and demand, to a certain extent, a modification in their treatment.

Having, in our notices of the former editions of Dr. Gairdner's treatise, adverted to the views inculcated by the author in regard to the pathology of gout and its hygienical and therapeutical management, we need only say, in reference to the present edition, that the practical observations on the particular application of remedies in the treatment of gout are very considerably extended; by which the instructions of the author are rendered more clear and explicit, and, consequently, more easy to be carried out by those who desire to test, by clinical experience, their true value.

An entirely new chapter is added, on what Dr. Gairdner very correctly terms "a subject of the keenest interest;" the existence of sugar as a healthy ingredient in urine, and of minute saccharine impregnations of this fluid as an attendant on various phases of gout.

Dr. Gairdner says that "it is with regret" he observes "the growth of this book." The additions that have been made to it, by which this growth has been occasioned, are of a character that will prevent his readers, we are convinced, from joining in this regret, inasmuch as they have increased its value in a greater ratio than they have its bulk. D. F. C.

ART. XXIV.—*The Diseases of the Fœtus in Utero, not including Malformations, with an Outline of Fœtal Development.* By HENRY MADGE, M. D., Member of the Royal College of Surgeons, Licentiate of the Apothecaries' Society, etc. etc. London, 1854. 12mo. pp. 200.

THE pathology of fœtal life is at once a curious and interesting subject of inquiry. Nor is it one altogether devoid of important results, affecting not merely the health and life of the fœtus, but also the well-being and comfort of the mother. That the diseases so frequently met with in the new-born infant—excluding, of course, certain forms of abnormal development—are the result of the diseased condition of one or other of the parents, but especially of the mother, or of morbific causes acting upon the latter, but the effects of which, in the development of actual disease, are experienced only by the fœtus in utero, cannot be doubted. Thus, children born of mothers labouring under syphilis, smallpox, tuberculosis, scrofula, intermittent fever, etc., are very apt to labour at birth under these same diseases; in other instances, however, infants are born with diseases, as acute and chronic inflammation of the peritoneum, of the lungs, or of the heart; purulent ophthalmia, or erysipelas, of which the mother, at no period during pregnancy, exhibited the slightest symptom. The pathology of these latter affections is a subject well worthy of investigation. Are they the result of morbid actions originating primarily in the structure of the fœtus,

or are they produced by morbific causes which act upon the fœtus through the system of the mother?

We possess few treatises devoted exclusively to a consideration of the diseases incident to the period of intra-uterine life, and these few are, for the most part, imperfect and unsatisfactory. Incidentally, the subject is noticed by numerous medical writers, and our professional journals contain several able and valuable communications on some parts of the subject, among which we would especially notice the two elaborate papers of Dr. J. Y. Simpson, of Edinburgh, on peritonitis occurring during fœtal life, and on some of the presumed consequences of intra-uterine inflammatory action.

In 1840, Dr. Wm. C. Roberts, of New York, presented a very interesting and fair exposition of the various facts recorded in reference to the diseases of the fœtus, as "A Contribution towards a better Acquaintance with Intra-uterine Pathology." (See the numbers of this Journal for August, 1840, and October, 1841.)

Dr. Madge must have entirely overlooked this contribution of Dr. Roberts, as well as the very elaborate memoir of M. Andry, on the diseases of the fœtus and its appendices, which occupies some eighty-two pages of the first volume of the *Journal des Progrès* (2me series, tom. i. 1830, pp. 126–205), when he penned the declaration that "nothing has yet appeared, having any pretensions to an elucidation" of the whole subject, "or to an arrangement of what is already known" in relation to it. "That as yet, we have had no clear and concise *résumé* of the important researches made by modern pathologists, and particularly by our continental brethren, on the diseases of the fœtus in utero."

The essay of Dr. Madge is certainly a most interesting one. He has collected, with commendable industry, the leading facts bearing directly upon the subject, and has endeavoured, as far as possible, to give to them a systematic arrangement, so as to indicate their pathological relations.

The information communicated will amply repay the time spent in the perusal of the work. The author has by no means exhausted the subject; his labours may, however, prompt to the investigation of the causes, character, indications, prevention, and cure of a class of diseases by which human life is impaired or destroyed even within the mother's womb. D. F. C.

Art. XXV.—*Principles of Comparative Physiology.* By William B. Carpenter, M. D., F.R.S., Examiner in Physiology and Comparative Anatomy in the University of London: Professor of Medical Jurisprudence in University College, etc. etc. With three hundred and nine wood engravings. A new American, from the fourth and revised London edition. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 752.

In the last edition, the principles of general as well as of comparative physiology were embraced in the same volume. The increased amount of matter not only deserving but demanding notice that had been accumulated, by the diligence of observers in various departments of the science, since the appearance of that edition, and the desire of the author to present a full exposition of the present state of the entire subjects embraced in the original work, has induced him to divide into separate treatises the principles of general and those of comparative physiology. For various reasons he has issued first the comparative physiology, which we have in the volume before us; it being his intention, as he informs us, to reproduce the general physiology, as a companion volume to this, so soon as his numerous engagements will permit him to bestow upon that portion of his revision the careful attention which it requires.

The treatise before us may be viewed in some measure as a new work. Every portion presenting not merely the indications of a careful revision, but copious additions and modifications, so as to render it a full as well as an accurate exponent of the actual condition of the science of comparative physiology.

The student of this highly interesting and instructive branch of natural history cannot but return his most grateful thanks to Dr. Carpenter for the preparation of a work so well adapted to facilitate his labours—in which, with so much skill and evident care, will be found orderly and methodically digested and arranged for his use, the contributions of many investigators, industriously accumulated from the numerous volumes in which they are found recorded—some of these volumes of difficult access, excepting to such as have at their command a large and well-supplied scientific library, while many of them are sealed books to the merely English reader.

With the work of Dr. Carpenter, now placed within his reach by the cheap, though splendid reprint of Messrs. Blanchard and Lea, no one need be deterred from the study of one of the most pleasing, as well as the most useful branches of natural science. By means of its clear and accurate teachings, all, whether professional or amateur students, may trace the wisdom of the Creator, in the structure and vital endowments of the various organized beings that people the earth; and in "that combination of unity of plan with variety of purpose, by which is produced the endless diversity united with harmony of forms, so remarkable in the animated world." D. F. C.

ART. XXVI.—*The Principles of Animal and Vegetable Physiology: A Popular Treatise on the Functions and Phenomena of Organic Life; to which is prefixed a General View of the Great Departments of Human Knowledge.* By J. STEVENSON BUSHNAN, M. D., Physician to the Metropolitan Free School, etc. etc. With one hundred and two illustrations on wood. Philadelphia, Blanchard & Lea, 1854. 12mo. pp. 234.

THIS is certainly a very pleasing book to read, and withal highly instructive. To such as desire to acquire a general idea of the principles of animal and vegetable physiology, but who have neither the time, nor perhaps the inclination, to derive it from the study of either of those extended systems of special or comparative physiology, in which the subject is fully treated in all its various relations and bearings, this little treatise of Dr. Bushnan is specially recommended. Concise as is the sketch which it presents, it is, nevertheless, sufficiently clear and accurate, throughout, to convey a definite and correct conception of the respective functions of the several organs of plants and animals, with the principal modifications of the organs and functions in the different classes of the latter;—while the leading phenomena of the development of the germ and embryo, in both divisions of the organic world, are pointed out. The results, also, of a complete analysis of the constituents of organic structure, are presented, whether as reduced to their component textures by the anatomist, or, by the chemist, resolved into their proximate and ultimate elements. In the introductory chapter "an attempt has been made to expound, in brief and lucid terms, the general nature, relations, and applications of all the chief departments of human knowledge, in order to give the reader, not specially trained in science, a general view of the vast field of inquiry which the Creator has laid open to the lawful exercise of the human intellect." And from this rapid survey of the various departments of knowledge, to point the student to the principles which ought to guide him in the pursuit and application of the several sciences.

We are far from recommending the treatise before us, as even a suitable manual for the use of those who would study thoroughly the science of physiology, either human or comparative. It is for the use of the general reader—of the student to whose future pursuits a profound knowledge of the subject is not essential, that the treatise has been written. For the purposes of such it is admirably adapted, and may be the means of leading many to a closer inquiry, through the aid of other guides, into the wonderful structure of all living organisms, and the nature and laws of their vital properties, actions, and relations. An

inquiry in which every one is interested, inasmuch as, from a correct knowledge of that structure and of those properties, actions, and relations, is derived our acquaintance with the means by which health and vigour may be promoted and maintained, and disease and premature decay avoided, not only in our own bodies, but in the animals and vegetables that furnish us with food and raiment—with the essentials, as well as with the comforts and luxuries of life.

D. F. C.

ART. XXVII.—*Healthy Skin: A Popular Treatise on the Skin and Hair, their Preservation and Management.* By ERASMUS WILSON, F. R. S., author of a "Treatise on Diseases of the Skin," "A System of Human Anatomy," &c. &c. Second American from the Fourth and Revised London Edition. With Illustrations. Philadelphia, Blanchard & Lea, 1854. 12mo. pp. 291.

THIS is, at once, a most interesting and instructive treatise, on a subject in reference to which no individual can be indifferent, inasmuch as upon a proper understanding of the structure and functions of the skin and its appendages, and of the means by which their proper and uninterrupted action can alone be maintained, the comfort and health of the body, as well as its exterior beauty, will in a great measure depend.

It has been well remarked by Hufeland, in his work on the art of prolonging life, that the skin "is not to be considered merely as a common covering to defend us from the sun and the rain, but as one of the most important organs of our body, without the incessant activity and agency of which there can be neither health nor long life, and in the neglect of which, in modern times, lies the secret source of numberless diseases and evils that tend to shorten our existence." How proper, therefore, is it that every one should make himself acquainted with the structure and offices of the skin, and the influence exerted upon it by diet, clothing, exercise, ablution, and bathing. In the work before us, these several subjects are discussed with great clearness and accuracy; and so well convinced are we of the importance of the author's teachings to all who would desire to insure and promote their own health and comfort, as well as the health and comfort of those placed under their control, that we should be pleased to find a copy upon the reading-table of every family in our land.

The proper care of the hair, though not, perhaps, attended with results commensurate with those which are derived from a due attention to maintain the healthful condition of the skin, is nevertheless of sufficient importance to claim the attention of every prudent person. The beauty and permanence of the hair, and the freedom of the scalp from troublesome, disgusting, and even dangerous diseases, is to a much greater extent under our control than is generally supposed. The remarks of Mr. Wilson in relation to this subject cannot fail to interest every one. The same remark may be made in reference to his concise but pertinent directions for the management of the nails. D. F. C.

ART. XXVIII.—*The Science and Art of Surgery: being a Treatise on Surgical Injuries, Diseases, and Operations.* By JOHN ERICHSON, Professor of Surgery in University College, and Surgeon to University College Hospital. Edited by JOHN H. BRINTON, M. D. Illustrated by three hundred and eleven engravings on wood. Philadelphia, Blanchard & Lea, 1854. 8vo. pp. 908.

THE high character of this excellent work is very fitly and forcibly expressed by the motto and title adopted by its author. He proposes to treat of the science as well as art of his favourite branch; and his motto is in the words of the philosopher of Verulam: "They be the best Chirurgeons which, being

learned, incline to the traditions of experience, or being empirics, incline to the methods of learning." We have here, in very few words, our author's idea of a true surgeon, and we are led at once to look for, what we soon begin to find in the pages before us, the production of an expert equally accomplished in the teaching and the practice of his profession. This book has been for some time before the public, and is already so firmly established as a most desirable companion to the analogous volumes of Fergusson, Druitt, Miller, Pirrie, and others of similar date, as to require no further introduction at this late hour. In fulness of practical detail, precision and perspicuity of style, convenience of arrangement, and soundness of discrimination, as well as fairness and completeness of discussion, it is better suited to the wants of both student and practitioner than either of its predecessors. In one material respect, moreover, it decidedly excels them all; for without sacrifice of essential matter, or needless display of novelty or useless lore, it affords a clear, concise, and faithful picture of the latest progress of its subject in most questions of really practical or scientific moment. As a compend of the existing state of British surgery, from one of the best schools and ablest expositors in the English metropolis, it is especially valuable in our view; and we the more regret, on that account, our entire inability, within the restricted limits of this notice, and in the short interval of time allowed us, to make any use of the numerous passages which have suggested themselves as worthy of especial note, in passing, for the benefit of those of our readers who may not have met with the original. We must content ourselves, therefore, with the strong recommendation of this original, as the most reliable text-book for the student and work of reference for the practitioner, among the English manuals of the present day. A brief outline of its plan of arrangement is all that we have room to add. According to the preface, Mr. Erichson's treatise contains the substance of the lectures on surgery delivered by him, as professor of that branch, at University College. Omitting all consideration of affections of the eye, and almost entirely that of skin and ear diseases, as neither of them come properly or practicably within the province of a systematic lecturer, he begins his course under the head of "First Principles," with a brief examination of the nature and treatment of inflammation, some appropriate and useful remarks on operations in general, and a pretty full account of amputations in particular. He then passes on, in a second division, to the study of surgical injuries, including among others, wounds, fractures, dislocations, and their operative and otherwise remedial treatment, under which head he occupies some two hundred and twenty-four pages, in twenty-two extremely interesting and instructive chapters. The third and last division is devoted to the history and treatment of surgical diseases, and presents in thirty-eight chapters and five hundred and seventy-three pages, an imposing array of the various ills that flesh is heir to, which would be vastly more formidable were it not accompanied by such an admirably effective display of the wonderful resources of the healing art. The notes of the editor, although not very full, are generally to the purpose, and carefully written. They supply some occasional omissions of the author, or relate to a few improvements and peculiarities of practice in this country, or for the most part in his native city, and appear to be generally suggested by the precept or example of his Philadelphia preceptors. The publishers display their usual liberality and taste in the getting up of the edition, and, in presenting it in such a handsome and convenient form, have conferred upon American readers an obligation which we trust will meet with a suitable response. E. H.

QUARTERLY SUMMARY

OF THE

IMPROVEMENTS AND DISCOVERIES

IN THE

MEDICAL SCIENCES.

ANATOMY AND PHYSIOLOGY.

1. *On the Spleen, and some Peculiarities of its Capillary System.*—Fuhrer considers the spleen to be at the same time an organ of *retrogressive metamorphosis*, as also a *laboratory* of the *blood-globules*. Concerning the first function, the spleen acts like the other secreting glands, only with this exception, that the products of the metamorphosis are not excreted by a proper duct, but are carried off with the blood by the vena lienalis, and conducted to other organs of secretion, to undergo there further change. The blood-globules are formed, according to Führer's view, within a peculiar adventitious system of capillaries. These adventitious transitory vessels originate from the normal capillaries themselves, analogous to the new vessels in pseudo-membranes. They commence as small excrescences, which elongate into thin processes, forming on some points of their course various swellings, cellular cavities, in which the blood-globules are developed. New ramifications proceed from these, forming anastomoses with others, and communicating through these with veins, into which they pour out their contents. As soon as the circulation and formation of blood becomes sluggish, these adventitious vessels collapse, and a part of them perishes altogether; on the other side, their development becomes very active, connected with swelling of the whole spleen, and increase principally of the Malpighian bodies during and soon after the process of digestion—*i. e.* during the period of a more energetic tendency of the blood to the spleen, with increased activity in the formation of blood. The peculiar fibrous cells (Faserzellen) described by other authors as forming a part of the parenchyma of the spleen, are cells or cellular bodies of the just described adventitious vessels; they are, therefore, fragments of the apparatus for the formation of blood, the nuclei seen within them represent the commencement of new blood-globules. As corroborating his view, Führer refers to the phenomena consequent on the extirpation of the spleen—*i. e.* vicarious swelling of the lymphatic, principally mesenteric, glands, which exhibit an unusual development of the normal capillary vessels, and besides this, the adventitious vascular system just described in the spleen.—*Brit. and For. Med.-Chir. Rev.*, from *Vierordt's Archiv.* Band xiii.

2. *Functions of the Liver and Spleen.*—According to Liebig, the bile is an intermediate product, by means of which certain elements of the body are transformed into carbonic acid and water. If this doctrine be true, it ought to follow that animals, from which the liver has been removed, should give off, in a given time, and in proportion to their weight, less carbonic acid than animals in which the organ has been left intact. Dr. Moleschott removed the liver from a number of frogs, and succeeded in getting a few of them to survive the operation. The most of the surviving frogs lived only three days, but some of them

even for fifteen days. Moleschott then placed the frogs thus mutilated into an ingenious apparatus, which only admitted sufficient air to maintain their respiration, and in which the carbonic acid expired was absorbed by a contrivance of Liebig's, and transformed into carbonate of potash.

He began his experiments by placing in the apparatus as many sound and uninjured frogs as weighed, collectively, 100 grammes.

The carbonic acid exhaled by these 100 grammes of frogs, in 24 hours, averaged 430 millegrammes. He next experimented with 100 grammes of frogs deprived of their livers, and found that, in the same space of time, they only excreted, on an average, 192 millegrammes of carbonic acid. The author chose for his experiments, as far as possible, animals similar in their age, sex, condition, etc. He also extended his researches, in a similar manner, to animals deprived of their spleen, and found that the quantity of carbonic acid expired by them, in a given time, was to the amount of the gas given off by uninjured healthy frogs, as 1 to 1⅔, thus proving that the spleen has less influence than the liver.

Moleschott also observed, that the blood of those frogs from which he had removed the livers contained *no sugar;* and this fact he considers confirmatory of Bernard's opinion, that, in the production of sugar, the liver does not act as a simple secreting organ, separating the sugar from the blood, as urea is thrown off by the kidneys. Indeed, we know that, after the removal of the kidneys, the blood becomes surcharged with urea.—*Monthly Journ.* July, 1854, from *Müller's Archives*, 1853.

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3. *The Influence of Dilution of the Blood on the Urinary Secretion.*—Dr. Kierulf made a section of the left ureter in various dogs, and he collected the urine secreted by means of a glass tube. This operation was performed on the first dog between 8.55 and 9.40 A.M., with very trifling loss of blood. Shortly before the operation, the dog passed a considerable quantity of normal straw-coloured urine. At 9.45 a small quantity of blood was abstracted from a vein; and at 11.40, 495 grammes of distilled water, at the temperature of 32° R. (104° Fahr.) were injected into the jugular vein. Five minutes thereafter, the urine was mixed with a considerable quantity of blood, and its amount was also increased. A second bleeding was performed at 11.55, and the urine was collected till 4.9 P.M., when it contained blood, but in a very small quantity. At 4.50 the dog was bled for the third time, and at 5 o'clock some sanguinolent urine was passed by the intact ureter. Next day the urine was of the normal clear straw-colour.

Other experiments of a like nature gave analogous results. He instituted other researches to ascertain whether the mixture of blood in the urine was due to an increase of the pressure of the circulating fluid, or to some change in its composition. He found that the urine passed after an injection of defibrinated blood into the veins, was perfectly clear. Dr. Kierulf makes the following deductions from his experiments:—

1. That *considerable dilution of the blood causes at first the excretion of albumen by the kidneys, and thereafter an admixture of blood in the urine.* This latter effect he regards as due neither to increased pressure of the blood, nor to rupture of the capillaries in the kidneys.

2. That the injection of water into the circulation increases the salts in the blood, although it diminishes the solids in the urine.

3. That the quantity of urine secreted in a certain time does not correspond to the proportion of water in the blood.

The author considers that these experiments, or others analogous to them, may yet cast light upon the obscure pathology of Albuminuria.—*Ibid.*, from *Zeitsch. für Rationelle Medicin, von Henle und Pfeuffer.* Bd. iii. 1853.

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4. *Blood-Crystallization.*—L. Teichmann has succeeded in obtaining crystals from blood without any preparatory evaporation, by the addition of four, five, or more parts of water to one of blood, and allowing the fluid to stand sufficiently long. In this way, and by the insertion of a small piece of cork under one of the angles of the covering glass, he has produced crystallization in the

blood of all the animals he examined, and in all the bloodvessels indiscriminately. His observations were conducted on the blood of man, oxen, swine, rabbits, pigeons, and fish; frogs' blood, for a time, formed the only exception. This observer believes that the crystallizable substance is contained in the blood-corpuscles; he has procured crystals from the filtered washings of the blood-cake, and states that he found them more perfect the more the corpuscles were freed from serum and fibrin. With regard to the influence of temperature, he has observed that the slower the evaporation takes place, the more complete will the crystals be; but if it be required to produce them quickly, the temperature may be slightly raised, but it must not be carried to the point at which the albumen coagulates. In subsequent experiments on frogs' blood, he procured crystals by the addition of a very considerable quantity of water, at a very low temperature; the quantity of the crystals was proportionately smaller than in other specimens of blood, and they are always colourless when thus obtained. From blood four months old, and also from dried blood, he has succeeded in forming crystals.—*Brit. and For. Med.-Chir. Rev.* April, 1854, from *Zeitsch. für Rat. Med.* Bd. iii.

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5. *Hämin Crystals.*—By this name Teichmann designates crystals obtained by treating the mass of dried blood-corpuscles in hydrochloric, acetic, oxalic, tartaric, citric, or lactic acids, under a glass cover in a temperature of 20° to 50° R. These crystals have a yellow, brick-red, brown, or black colour; they form rhombic prisms, or sometimes stellæ, needles, or granules, like those of black pigment. They are insensible to the action of air, and are insoluble in water, ether, alcohol, acetic, hydrochloric, and nitric acids; in boiling nitric acid, however, they are entirely dissolved; they are soluble in dilute liquor potassæ, but become blackened in this fluid when concentrated; concentrated sulphuric acid, and still more readily ammonia, dissolves them. To the substance which undergoes this form of crystallization, and which he supposes to be derived from the blood-corpuscles, he gives the name of Hämin; he considers it to be different from the hæmatin of fresh blood, which is soluble in water, and also from the hæmatoidin forming the crystals described by Zwicky and Virchow. He has obtained Hämin crystals in the blood of various animals. Finally, he considers the black pigments as forms of undeveloped crystals in different degrees of oxidation. As a practical result of the knowledge of the properties of Hämin, he suggests the employment of the above manipulations to obtain crystals in cases of suspicious spots and stains.—*Ibid.*

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6. *Influence of Cod-Liver Oil and Cocoa-Nut Oil on the Blood.*—Dr. Theophilus Thompson read before the Royal Society (April 27, 1854,) a paper on this subject.

He found, that during the administration of cod-liver oil to phthisical patients their blood grew richer in red corpuscles, and he refers to a previous observation of Dr. Franz Simon to the same effect. The use of almond-oil and of olive-oil was not followed by any remedial effect; but from cocoa-nut oil, results were obtained almost as decided as from the oil of the liver of the cod, and the author believes it may turn out to be a useful substitute. The oil employed was a pure cocoa oleine, obtained by pressure from crude cocoa-nut oil, as expressed in Ceylon and the Malabar coast from the Copperah or dried cocoa-nut kernel, and refined by being treated with an alkali, and then repeatedly washed with distilled water. It burns with a faint blue flame, showing a comparatively small proportion of carbon, and is undrying. The analysis of the blood was conducted by Mr. Dugald Campbell. The whole quantity abstracted having been weighed, the coagulum was drained on bibulous paper for four or five hours, weighed, and divided into two portions. One portion was weighed, and then dried in a water-oven, to determine the water. The other was macerated in cold water until it became colorless, then moderately dried, and digested with ether and alcohol, to remove fat; and, finally, dried completely, and weighed as fibrin. From the respective weights of the fibrin, and the dry clot, that of the corpuscles was calculated. The following were the results observed

in seven different individuals affected with phthisis in different stages of advancement:—

		Red corpuscles.	Fibrin.
First stage, before the use of cod-liver oil ...	Female	129.26	4.52
	Male	116.53	13.57
First stage, after the use of cod-liver oil ...	Female	136.47	5.00
	Male	141.53	4.70
Third stage, after the use of cod-liver oil ...	Male	138.74	2.23
Third stage, after the use of cocoa-nut oil ...	Male	139.95	2.31
	Male	144.94	4.61

Med. Times & Gaz. June 10, 1854.

MATERIA MEDICA AND PHARMACY.

7. *Experiments on the Preparations of Squill.*—Dr. Chateau has instituted a series of experiments, with the view of replying to a question proposed by the Faculty of Medicine of Paris: "*Determine by clinical observations what are the effects of the preparations of squill.*" In a *résumé* of the memoir presented to the Academy, M. Chateau states the powder, wine, vinegar, and oxymel of squills were employed; but the powder, most frequently, on account of its action not being interfered with by any other constituent of the medicine.

His first *experiments were made upon seven dogs*, to whom he administered quantities of the squill varying from 40 grammes of the powder to 1 gramme, or their equivalent, of the wine of squill. In one of these experiments, 1 gramme of soft extract of squill was injected into the subcutaneous cellular tissue; in all the other experiments, the drug was introduced into the stomach. The effects produced were the following: The animals became dull, and this was followed by increase of the buccal secretions, nausea, efforts at vomiting, and by semi-liquid stools passed in small quantities; when the dose was sufficiently large, tremblings supervened, and paralysis of the posterior limbs, which soon extended to the anterior. After this, the animals appeared to regain their equilibrium, and then suddenly a convulsive attack occurred; they fell upon their side; there were some movements of deglutition, a little orthotonos, and death in from thirty-five minutes to one hour and fifty-five minutes after administration of the drug. When the dose was small, the same series of phenomena was exhibited, but more tardily; and death was delayed for twelve or fifteen hours. In all the experiments there was a remarkable diminution of the temperature of the body, as taken in the rectum, believed to be due to the hyposthenic action of the squill upon the nervous system. On *examination of the bodies after death*, the viscera were found congested, the blood black and thick, the bladder empty, or containing but little urine, the ganglia of the great sympathetic reddened, the cerebrum little altered, but the cerebellum and spinal cord softened sometimes even to diffluence.

M. Chateau next relates the results of his *observations on the human subject in disease*, which had reference principally to the diuretic and laxative operation of the drug. He administered it in 4 cases of pulmonary emphysema, in 3 cases of albuminuria, 3 cases of abdominal dropsy, 1 of which was ovarian, 2 cases of pleurisy, 2 cases of pneumonia, 2 cases of phthisis, and several cases of rheumatism, only 1 of which is related in the original memoir. In these 17 cases, the operation of the squill was as follows: in 7, it proved *diuretic and purgative;* in 2, simply *diuretic;* in 2, simply *purgative;* in 4, *expectorant;* in 1, *diuretic and expectorant;* and in 1 no effect was observed.

He noticed that when either the purgative or diuretic operation became excessive, the other immediately ceased.

The number of cases of each disease in which the squill was administered was too small to warrant the general therapeutical conclusions drawn by Dr. Chateau; we shall therefore content ourselves with noticing two points of in-

terest connected with the operation in the cases described. Of the cases of *albuminuria*, 2 were purged by the powdered squill without any increase in the secretion of urine; in the third case, where the wine of squill was employed, a diuretic effect followed; but in all three cases, the albumen in the urine was unaffected. In one of the cases of *abdominal dropsy*, where the powder proved diuretic and laxative, the dropsy was removed in less than two months.

The reply of Dr. Chateau to the question of the Academy is: "*That squill has a direct hyposthenic action upon the ganglionic and cerebro-spinal systems, and that this action is exhibited at first by an increase of the urinary and intestinal secretions, and at last, if the dose of the drug be large, by paralysis and death.*"

Dr. Chateau prefers the powder for internal administration, next to this the wine of squill, and as an expectorant, the oxymel. He considers 10 to 15 centigrammes (from 1½ to 2¼ grains) a good *dose* to commence with, and that it may be increased, after some days, to 35 or 40 centigrammes (5¼ to 6 grains). —*B. and F. Med.-Chirurg. Rev.* July, 1854, from *Archives Générales*, Jan. 1854.

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8. *On Delphinin.*—The deficiency of our knowledge respecting the properties and operation of the alkaloid delphinin, has led Dr. Leonides van Praag to their new and complete investigation. The delphinin which was employed in the experiments whose results we are about to detail, was obtained from Messrs. Frommsdorf, of Erfurt.

Dr. J. L. Van Praag administered the alkaloid to animals from each class of the vertebrata, and in the paper under our notice, gives the results of his observations on fishes, frogs, birds, and mammalia, and relates at length the experiments which he instituted. The mammals which he selected for experiment were dogs, cats, and rabbits. In these, asphyxia, as a result of paralysis of the heart (*Herzlähmung*), was among the principal phenomena; whilst paralysis (*Lähmung*) of the nerves of motion, and, at a later period, of those of secretion and the special senses followed. Dogs were least of all affected when the poison was given to them in a piece of meat, since the two were soon vomited together. It operated more powerfully when it was administered by the anus, or introduced into a wound of the integument. But death followed almost suddenly, so that scarcely a minute intervened, upon injection into a vein, from arrest of the heart's action; the animals, in consequence of discontinuance of respiration, gasp for breath with open mouth, and in a short time death follows, in the midst of severe tetanus. On injection of the alcoholic solution into the anus in dogs, repeated evacuation of feces at first occurred, and then salivation. The walk became staggering and uncertain. There arose great adynamia, in consequence of which they leaned against anything for support, until at last they fell down. The sensibility of the skin diminishes simultaneously with the lessening power of the motor nerves, until anæsthesia is complete. The respiration is at first increased, panting and accompanied by howling; in addition to this, tenacious, clear mucus accumulates in the glottis and larynx, which renders respiration still more difficult. At a later period, the frequency of respiration diminishes, until at last the act occurs very rarely, or ceases altogether. The functions of the brain and of the organs of the senses are little affected, until the disturbance of the circulation and respiration increases. In the instance of cats, into whose anus the alcoholic solution was injected, the evacuation of feces and flow of saliva were also increased, and there were observed, at first, frantic and wild leaps, the symptoms of adynamia occurring at a later period. The phenomena of the circulation and respiration are the same as in dogs, and both cats and dogs show signs of a tickling sensation in the mouth. In rabbits, also, after the injection into the anus, active and strong symptoms of reaction appear, as in cats, terminating, however, in adynamic symptoms. When the delphinin, in a fatal dose, was introduced, in cats, into the subcutaneous cellular tissue of the back, the poison operated first of all on the sensory nerves at this spot, and induced symptoms resembling those arising from a powerful irritant. The animals exhibited great disquiet, bristled up their hair, set up their back, rolled on their back, and finally lay down. Gradually, however, in this case also, the action of the poison upon the

heart and lungs appeared. When, in the instance of dogs, the poison was given dry with the food, the operation of the poison soon became apparent upon the mucous membrane; there were observed vomiting, and an excessive itching in the mouth, so that the animals rubbed their lips and mouth with their feet, and rubbed their nose upon the ground. With this, also, arose profuse salivation. The principal points observed on dissection of the poisoned animals were, fulness with blood of the membranes of the brain, of the heart, of the larger venous trunks, and of the liver; fulness of the gall-bladder, and a collection of mucus in the air-passages. Nothing was noticed respecting the condition of the kidneys. In one case, air was found in the veins of the membranes of the brain.—*B. and For. Med.-Chirurg. Rev.* July, 1854, from *Archiv. für Pathol. Anat. und Physiol. und für Klinische Medicin*, Bd. vi. Heft 3.

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9. *Comparison between Chloroform and Cold as Anæsthetics.*—By James Arnott, M. D. The last number of the *Medical Times and Gazette* records another death from chloroform. Within the last three months, three deaths have happened from the same cause in the London hospitals alone, and in the hands of careful men, experienced in the use of this agent. Yet, as the fatality is small in comparison with the number of cases in which chloroform is used, every one *hopes* that such an event will not happen in *his* hands. Is it proper, however, to incur any amount of risk when the same object—the prevention of pain—can be attained by other means perfectly devoid of danger, and possessing many other advantages? Does not the surgeon err who continues to use chloroform unnecessarily, even should he himself never so cause death, by countenancing the employment of a measure that every month proves fatal in the hands of some other surgeon? Does he not, in fact, so participate in causing these deaths? It is merely a question of time. The moment some eminent patient dies from chloroform, unnecessarily administered, that moment will it be deemed imperative to substitute a safer anæsthetic. Why should not this time be anticipated, and such an event prevented? Why should not a stop be at once made to the afflicting mortality from this cause? For what can be more distressing to the relatives of a person so killed, or to the surgeon who administers the fatal poison, than the consciousness that its administration was altogether unnecessary.

Benumbing cold has been proved to be a safe and perfect anæsthetic in the immense majority of surgical operations. It would have prevented death in more than three-fourths of the fifty or sixty cases that have proved fatal from the use of chloroform. Among others who have afforded evidence of these powers of intense cold, I may mention M. Velpeau, in France, and Mr. Paget, in this country. The French journals have at various times reported M. Velpeau's use of cold for this purpose in the *Hôpital La Charité;* and the *Medical Times and Gazette* of the 1st inst. reports an operation in which it was successfully employed by Mr. Paget. In a paper, in the *Medical Times and Gazette* of the 25th of March, I have mentioned how cold is applied to produce anæsthesia; the design of the present communication is to make a comparison between it and chloroform.

It is commonly supposed that the application of benumbing cold must be a difficult and troublesome proceeding; much more so, in both respects, than the administration of chloroform. The very contrary is the truth. Whether the cold is applied by keeping in contact with the part, for a few seconds, a refrigerating mixture of ice and salt, contained in a gauze bag or a thin metallic vessel, or by touching it with a thick piece of copper that has been dipped in such a mixture, nothing can be easier; and it is impossible to fail. Different from chloroform, the anæsthetic effect is complete within a minute; and, as it has no unpleasant consequences, the surgeon is released from those protracted attentions which he is so often called upon to give in allaying the nervous symptoms that frequently follow the administration of chloroform. He requires no assistant; and, as the anæsthetic brings no new danger of its own, his mind is undisturbed, during the operation, from the anxiety which he would suffer from chloroform on this account.

The expense of either plan is so trifling that it does not deserve mention with

respect to private practice; but, with reference to hospitals, where the strictest economy is required, it may be worth while to state that cold does not cost a twentieth part of the price of chloroform. In using a frigorific mixture for remedial purposes in dispensary practice, I have made two pennyworth of the materials answer for several cases in succession. Mr. Ferguson, of Giltspur Street, has had benumbing vessels elegantly made of silver; but, however well suited for private practice these may be, a rougher apparatus will answer. On one occasion, in employing congelation for phlebitis, I borrowed for the purpose the net which confined the hair of the attendant nurse; and the principal ingredient cost as little as the instrument which contained it, for, there being a snow-storm at the time, it was gathered from the door-step.

The perfect safety from cold, and the anæsthesia from chloroform in the deepest operations, are the great respective advantages of these agents. Of the thousands of times intense cold has been used, not once has it been followed by any more untoward event than a slight cutaneous irritation. If the skin is merely benumbed, no redness follows the application; if congelation of the adipose matter under the skin is caused, a redness comes on, which may continue for a day or two. But, as explained in the paper already referred to, in the *Medical Times and Gazette*, this is the very contrary of inflammation. Instead of being a symptom of inflammation, the redness shows that a condition of the part exists rendering inflammation impossible. And in this safety produced by congelation, there is an advantage not inferior in importance to the insensibility. For, to the erysipelas and phlebitis following surgical operations, the greater number of deaths occasioned by them is to be attributed.

The anæsthesia from chloroform in deep operations can only be called perfect under the supposition, still contested, that the unconsciousness of the patient afterwards, that he has submitted to an operation, proceeds from having felt no pain, and not merely from having forgotten it. To judge from his struggles and cries, the latter would be the conclusion.

The anæsthesia produced by chloroform is by no means so certain as the anæsthesia produced by cold, because, in the latter case, there is no unconsciousness. But, in deep operations, it is only the incision of the skin which is very painful. The most eminent orthopædic practitioner of the day states, in a letter to the writer, that in the operations he is conversant with, the only source of pain is the incision of the skin; and perhaps no surgeon has had so good an opportunity of forming an opinion on this point. But all will agree that, if the insensibility of the skin were suspended, there would be very little suffering from the cutting of the deeper parts. So little, indeed, that it becomes a question whether life should be endangered by suspending it. The pain attendant on tightening the ligatures of arteries could be easily obviated by the momentary previous application of a congealing copper ball.

Chloroform, by causing unconsciousness, prevents the patient's assisting the surgeon in his operation, and apprising him of mistakes that may happen in its performance. The public has just been reading, with horror, the account of attempts made to drag a stone from an unopened bladder, by a forceps introduced through the wound, and grasping both stone and bladder. But for the insensibility induced by chloroform, the screams of the unfortunate child would at once have indicated the error; and the system, perhaps, is more to be blamed than the surgeon.

In the act of administration, and afterwards, certain inconveniences attend both measures. Chloroform, besides producing unconsciousness, causes a sensation of choking, and is often succeeded by headache, sickness, and prostration. Cold, applied only to the degree of benumbing (which may often be sufficient), causes no unpleasant sensation; but when congelation is produced, there is a sense of pricking, like that caused by mustard, both at the time, and after the return of the circulation. This subsequent smarting may be entirely prevented by a moderate application of cold; and that which first takes place may be lessened, if thought worth while, by a little management.

In recapitulating the subject, we may say, that although, in deep operations, the insensibility produced by chloroform may be greater than that produced by cold (unless this were applied in the successive stages of the incision), in all

superficial operations, which constitute the immense majority, cold is superior to chloroform in the circumstances of safety, ease of application, or the saving of time and trouble, certainly of producing anæsthesia, and, lastly, in the power it possesses of preventing subsequent inflammation. Surely, a conscientious and humane surgeon will not allow the prejudice against novelty or innovation to outweigh so decided a superiority. Anæsthesia will, no doubt, henceforth be a required element of every surgical operation; but chloroform, fortunately, is not the only mode of producing it.—*Med. Times and Gaz.* Aug. 5, 1854.

10. *Charcoal Coverlet for the Prevention of Smell from Gangrenous Sores.*—In some cases of hospital phagedæna recently under his care, in St. Bartholemew's Hospital, Mr. Wormald made an ingenious and very useful application of the disinfecting powers of charcoal. It is well known that dry charcoal will effectually absorb any noxious or offensive gas which can be made to pass through it. On this power, Dr. Stenhouse's disinfecting respirators depend for their efficiency. The difficulty in applying it in hospital practice has, however, arisen from the difficulty of keeping it at the same time dry and in a uniform layer around the part giving rise to effluvia. Dr. Wormald's plan consists in sprinkling freely between two sheets of cotton wool a tolerably thick layer of powdered charcoal, and then "quilting" them together in small segments, so that the powder is retained securely in its place. The pads thus prepared may be of any size, according as required to wrap round the end of a stump, or to cover a superficial ulcer. The sloughing sore having been dressed in the ordinary manner, and a little lint or wool so placed as to absorb any discharge which may flow, over all is laid the charcoal quilt, which is then lightly confined by a bandage. It forms, in addition to its disinfectant properties, a very soft and comfortable envelop, more especially if the sore be in such a part that the patient is obliged to lie on it.—*Med. Times and Gaz.* July 1, 1854.

MEDICAL PATHOLOGY AND THERAPEUTICS, AND PRACTICAL MEDICINE.

11. *Treatment of Fever by large Doses of Quinia.*—Dr. F. Corbyn states that from his long experience of the treatment of fever by large doses of quinia, when this disease broke out with such violence in its typhoid form at Anarkallie and the Citadel, he urged on the attention of medical men, when he went through their hospitals in August, the immediate necessity of prescribing quinia in large doses, as twenty grains, for instance, in each. Their apprehension was that the expenditure by such a practice would be so great that they could never obtain an adequate supply; but he soon allayed their fears, by demonstrating the fact that the expenditure would be 50 per cent. less than in the ordinary practice of administering frequent and, after all, useless small doses of that invaluable medicine. One or two doses, he asserts, when large, will eradicate the fever at once, if combined with good effective purgatives. He found that in H. M.'s 90th, small doses were being prescribed; but that in the Horse Artillery, on the contrary, the very opposite treatment was pursued.

Dr. Mackinnon thus remarks on the success of this method; his success was wonderful:—

"The cases of intermittent fever were so numerous, that I had full opportunity of trying every mode of administering quinia, and I have come to the conclusion that the most effective and the most economical mode of administering it, is to give it in a single large dose, at or towards the termination of the sweating stage. It is now my practice to give ʒss at that period, and I have never seen it fail to put a stop to the disease at once. I have given the same quantity six or eight hours before the accession of the paroxysm; but I found its effect uncertain; it often checked the paroxysm, but it sometimes failed to do so; in the latter case, however, though one paroxysm succeeded the dose, I

never saw a second follow. It was this circumstance which led me to conceive that the period of six or eight hours was too short for the full development of the antiperiodic effects of the medicine, and I was thus induced to prescribe it at the termination of the sweating stage. I have found it equally effective in tertian as in quotidian intermittents.

"Lesser doses of from twenty to twenty-five grains, I have found so often to fail, that I adhere now, in every instance, to the large dose.

"I have not found it to produce vertigo, tinnitus, or other cerebral disturbance in a greater degree than small doses frequently repeated do; in fact, as the period of administration of the medicine usually occurs late in the evening, the patient gets his dose at bedtime, falls asleep, and if any sensations of vertigo, or tinnitus, or of other cerebral disturbance, are experienced in the morning, they are usually slight. I prescribe the medicine without much reference to the state of the bowels; if they are confined, a purgative is either given along with the dose, or deferred till the succeeding morning, as the case may seem to require. If they are easy, I often give no medicine but the quinia.

"The great recommendations of this mode of administering quinia in hospital practice are: 1. That it economizes the expenditure of the drug: in the usual plan of giving quinia in repeated doses, from forty to fifty grains and upwards are often consumed in the treatment of a single case; rarely, in the intermittents of hot climates, so little as thirty grains. 2. That the soldier returns to his ranks more rapidly. If the case is recent, and uncomplicated with local derangement, the patient is sometimes discharged on the second day after admission, but usually on the third day, an earlier period than was attainable on the old plan of treatment. Occasionally, admission into the hospital is not desired; but the patient comes in the evening, swallows his dose of quinia, and returns to the ranks. 3. That it makes fewer demands on the attention of the hospital subordinates, and diminishes greatly the amount of their labour, instead of having repeated doses at specific hours to administer, a matter, in an hospital full of cases of ague, involving no small labour, and requiring considerable attention as to time. There is only one dose to be given.

"In remittent fever, I have also, during the remission, prescribed a single large dose of quinia with the best effects; but the cases in which I have so prescribed it have been too few to admit of my giving an unqualified opinion as to the superiority of this over the common mode of administering the remedy."

Dr. Mackinnon did not conceal his treatment under a bushel, but communicated his success to his assistant, Dr. Mactier, in medical charge of the 3d troop of his brigade, which was then stationed at Loodianah, and where fever was prevailing to a fearful extent. Dr. Mactier thus describes the disease, and his success also:—

"The only disease which has prevailed to any remarkable extent in the troop, during the past year, has been intermittent fever; but from this they suffered so much as to attract the notice of government, and cause their removal from Loodianah to Jullundur. Occasional cases of fever occurred during the early part of the year; but it was not until August that the number became in any degree remarkable. From this time, however, they rapidly increased, till about the 20th of October, when the disease reached its maximum. On one occasion, twelve cases out of one hundred men were admitted into hospital in one day. and it was no unusual thing to have forty sick under treatment, besides ten to twenty convalescent; with only two exceptions, every individual in the troop, including women and children, has suffered from one or more attacks of fever. It is a subject of congratulation, that out of 356 cases of fever, treated during the season, only one proved fatal. The symptoms of the disease were the same as have generally been remarked. On its appearance, the cold stage was frequently absent; but this became more and more marked, and, latterly, it was the one from which the patient suffered most severely. In a few instances, the prostration of strength during the ague fit was so great that stimulants required to be liberally administered. The most remarkable feature in the disease was the obstinacy with which it returned, after the patient had suffered from one or two attacks. Affections of the spleen were, as might be supposed, exceedingly common, although, in general, they were only temporary. Still, in not

a few instances, the organ became chronically enlarged. Seven of the worst cases of the sort have been sent to Landour for the ensuing hot season.

"As regards treatment, the ordinary method of administering divided doses of quinia, was for some time had recourse to, and with the usual result; the disease was cured, but not until the medicine had been taken for several days. In October, I began to adopt the plan of giving one large dose of quinia, twenty to thirty grains, a few hours before the expected paroxysm, and the results were in the highest degree satisfactory. Latterly, however, I have tried another method, which, in my practice at least, has proved decidedly more successful, viz: instead of giving the large dose of quinia before the febrile paroxysm, I now administer it just after the sweating stage has subsided. Since this treatment was suggested to me, by Dr. Mackinnon, of the 3d Brigade H. A., in November, I have never thought of employing any other, as, out of upwards of fifty cases, in only two has it failed immediately to cut short the attack. I have never seen disagreeable symptoms result from the large doses of quinia; headache and giddiness have certainly occasionally been complained of, but not more frequently than when the small repeated doses were given. The simplicity of the treatment, the saving of trouble to the attendants, and the reduced expenditure of the medicine, are all recommendations in favour of this practice, which, though by no means a new one, has not, I believe, been generally adopted. In the treatment of natives, large doses proved alike economical and successful."

Phillour, like Loodianah, suffered unprecedentedly from this severe fever during the year 1850; admissions during July, August, September, October, and November, of fever cases were 1,751; died, 10. But, during 1851, Assistant Surgeon Frederick Corbyn, M. D., took medical charge. It was to be supposed, from the repeated attacks of almost every man in the regiment this year, that they would be more weakly and predisposed, and that greater mortality would ensue. Dr. Corbyn prescribed twenty-grain doses of quinia, accompanied with a brisk purgative; hence, though during July, August, September, October, and up to the 8th November, there were 1,037 fever cases admitted, not one died; but, beside the sick of the 49th Regiment, there had been a considerable number of fever cases among officers, conductors, and serjeants and their families, as well as among the arsenal establishment, in all of which recovery took place under the same treatment.—*Indian Annals of Med. Sci.* Oct. 1854.

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12. *Treatment of Epilepsy.*—Dr. R. B. Todd, in an interesting clinical lecture delivered at King's College Hospital, offered the following valuable practical observations on the treatment of epilepsy. In reference to the drugs which have been generally employed, he observes: "Most of these have been used in consequence of their exhibiting a certain amount of physiological influence on the nervous system. As to others, it is difficult to trace the circumstances which led to their admission among the *juvantia* in this disease. First in the list I would place those drugs which belong to the narcotic class—opium and its various preparations, belladonna, henbane, conium, camphor. These certainly exercise a very powerful influence in calming the excitable state of the nervous system which accompanies epilepsy. They are applicable chiefly to the more acute cases, and especially when epileptic delirium is threatened or present. You should never administer them except when you have full opportunity of watching your patient, or of confiding him to the care of another. This remark, I need hardly add, applies chiefly to belladonna and the preparations of opium.

Belladonna has been greatly extolled by some French writers, far more than it deserves, as far as I can judge from my own experience. It is a remedy which leaves no permanent ill effect, and which I think, is worthy of a more extensive trial in this country than it has yet received. That it exercises a physiological influence on the nervous system, no one can doubt, who has administered it for forty-eight hours; but as it induces a state of pupil very like that which is so common in epilepsy, one might fear that it rather favoured the epileptic state. A good series of clinical observations are yet wanting upon the effects of this drug. The preparation chiefly used in France is the powdered leaves of the plant.

Opium is often useful in cases in which the fit is apt to occur in the night or early morning. A full dose given at bedtime will prevent the development of the fit.

The valerian was used formerly in epilepsy more extensively than now. It is a medicine, however, which should not be despised or discarded. Its use promises most in the complications of hysteria and epilepsy. Its virtue depends upon the presence of an oil or acid which is capable of entering into combination with metallic bases, as zinc and iron, and which also combines with quinia and other alkaloids. You may give it either in scruple doses of the powder two or three times a day, or in infusion, or as an ammoniated tincture, or in combination with quinia, or the metals.

Other nervine remedies may be added to your list, and you should keep them in reserve, to employ as occasion may require, such as musk, castor, assafetida, stramonium, sagapenum, garlic, remembering the moral influence of a change of drug. I cannot say that the evidence of any special power of an anti-epileptic nature in any of these drugs is at all satisfactory.

In this class of drugs you may place the Sumbul, of which a tincture is prepared by Mr. Savory, of Bond Street. One of the earliest cases in which I employed it, seemed to benefit very decidedly by it; and I have since used it many times with unequivocal good, so as to lead me to look upon it as a useful stimulant, and antispasmodic remedy.

Digitalis has been greatly lauded by some. It may act favourably partly by its diuretic properties, and partly by its influence on the heart's action. There is quite sufficient evidence of its utility to warrant its being retained among the list of remedies applicable to this disease, and to justify its occasional cautious use.

The cotyledon-umbilicus may rank with digitalis as a remedy for epilepsy. I cannot say that I have met with a case distinctly benefited by its use; but the cases published by Mr. Salter, and those recorded by the late Dr. Graves, indicate that it possesses a certain anti-epileptic power. I incline to think it acts by a diuretic influence.

There are other drugs obtained from the vegetable kingdom, which, I think, deserve a trial in epilepsy. One of these is the Achillæa millefolium, which, in the form of a decoction, exerts a very decided diuretic influence. On the same principle, taraxacum may do good in certain cases, as in those in which the epilepsy has relation to gout in the system.

The alkaline salts, especially the bicarbonate and the nitrate of potass, appear to me often to be very useful on a similar principle, namely, by increasing the activity of the kidneys.

The cardamine pratensis was greatly extolled by Sir George Baker. Turpentine has been used chiefly from its anthelmintic properties. It is a remedy that should be used very carefully, and never, if there be suspicion of organic disease of the brain.

The *selinum palustre*, or *peucedanum montanum*, is highly commended by Herpin, on, as I think, insufficient grounds. The plant is very difficult to procure, and I have not yet been able to try it.

The various metallic tonics have been and are extensively used in epilepsy. Of these, the safest is zinc, the sulphate, or the oxide. Either of these preparations may be given without any disadvantage that I know of, for a considerable time, and in large doses. Some time ago I gave to a patient in this hospital as much as half a drachm of sulphate of zinc, thrice a day, without any sensible effect. The dose had reached that amount by gradual increase. Whether these drugs exercise any special favourable influence, I am unable to say; all I know is, that, under their long-continued use, patients sometimes cease to be troubled with fits.

The various salts of iron are also applicable to the treatment of this disease, and especially to cases in which the blood is poor in hæmatin. But they are not so harmless as the salts of zinc. Their tendency in some persons to excite headache precludes their use with them; and they often disturb digestion, and on this account cannot be persevered with. The saccharine carbonate, the sulphate, the citrate, and the tincture of the sesquichloride, as well as Griffith's

mixture, are the preparations of iron which you will find most suitable to the generality of cases.

Salts of copper and of silver are also used for epilepsy, with, I think, at best, but very doubtful efficacy. To the cautious use of the ammonio-sulphate of copper, I see no material objection; in too large doses it may irritate the gastro-intestinal mucous membrane, but such irritation is easily removed.

There is, however, a much more serious objection to the use of the salts of silver. The nitrate and the oxide are the salts employed. Now, there is no doubt that the nitrate discolours the skin. We see repeated examples of this in the streets; scarcely a day passes that I do not recognize persons in the streets, as epileptics, who had been treated with nitrate of silver. If the nitrate of silver were a certain, or even a very frequent cure for this formidable disease, well and good; but, seeing that it is very far from being entitled to the credit of being such a remedy, I say, that we have no right to make blackamoors of our patients, and to stigmatize them forever as epileptics, and too often *as epileptics uncured*, on the mere chance of doing good by nitrate of silver. No doubt few would hesitate long between choosing a discoloured skin, and being the subject of a disease so fearful as epilepsy. But it seems to me, that the remedy ought to be a specific, to justify the practitioner in administering it, with the strong probability there is of producing such an effect. Remember, too, that the nitrate of silver stain communicated in this way, is probably indelible, and that, by an experienced eye, it is not to be mistaken. I have seen it after ten and fifteen years from the administration of the drug.

Finding, then, no special virtue in the nitrate of silver as against epilepsy, and that it is very apt to produce the serious result to which I have referred, with whatever care it may be administered, I have long since abandoned its use. I fear the oxide of silver is not more promising, either as to its influence on the disease, or its freedom from the discolouring power.

Indigo has likewise been given in epilepsy, but with no other result, as far as I can learn, than what Penil observed, that it turned the nails of his patients blue.

The time has, I think, now arrived, when we must look in another direction for an anti-epileptic remedy. We must turn our attention more particularly to those substances which are capable of being exhibited by inhalation, of being brought to bear upon the nervous system, without being subjected to the action of the gastric fluid.

Ether and chloroform, it is now well known, are capable of being taken into the blood in this way. Some years ago, I tried experiments as to their power of controlling the convulsions induced by strychnine, and with the result of finding that they possessed that power very completely. As long as an animal is fully under the influence of ether or chloroform, no convulsion can be produced. I was thus led to try their inhalation in epilepsy. I have chiefly employed for this purpose chloroform, because it is less disagreeable to patients to take. The results of my observations so far, are these—that in the more acute forms of epilepsy, the inhalation of chloroform has considerable influence in controlling and modifying the attacks; it has also a very decided power over epileptic delirium; and over some of the concomitants of epilepsy, as the violent convulsive jerks of the muscles of both upper and lower extremities, which are always most distressing, and often very dangerous. It has a marked and immediate power over puerperal convulsions; and I am glad to see that my friend, Professor Simpson, of Edinburgh, sanctions, by his high authority, its use in such cases. It is also applicable, and with advantage, to the convulsions of infants, and to laryngismus stridulus.

There is no use in employing chloroform in epilepsy unattended by other symptoms, when the attacks are at uncertain times and at long intervals; nor do I advise you to attempt its use, unless the patient is fully under your control, or will heartily co-operate with you. And in cases where the heart is affected, either functionally or organically, it should never be used. Its inhalation at two or three stated times of the day, exercises a very calming effect on the nervous system, diminishing its excitability very materially. But this must be car-

ried on over a long period of time—many months, and even years; and the dose may vary from twenty to sixty minims, according to the effect produced, and you should aim at obtaining a gradual, not a rapid effect. The patient should always remain in the horizontal posture during the inhalation, and for at least half an hour after it.

You will expect me to say something on a proposal made within the last few years to open the trachea of patients suffering under severe epilepsy, with the twofold object of preventing the fits, or of obviating their effect, in creating undue distension of the cerebral bloodvessels, if they should occur.

Before a proposition of this kind can meet with general favour from practical men, it must be satisfactorily proved that the hypothesis, or theory, if you will, from which the proposal emanates, is essentially sound: and next, it must be shown that the operation is in itself one that is not seriously dangerous to life; and lastly, it ought to be in the highest degree probable that the attacks will not recur after the operation has been performed. It is, as you know, contrary to all principles of sound surgery to perform an operation, unless it be for the complete removal of the disease, or, at worst, to stave off the reappearance of the malady to the latest period.

The hypothesis from which this proposal emanates assumes that the exciting cause of all the evil in severe epileptic paroxysms, is the contraction of the muscles of the neck and glottis, which induces and maintains a congested state of brain, which, in its turn, excites and keeps up the convulsions. I have elsewhere examined this congestion theory in detail, and I am not aware that the arguments which I then advanced against it have ever been fairly met. I have also shown, by experiment, that epileptic convulsions may be excited in dogs which had been freely tracheotomized previously, full provision having been made for the free ingress and egress of the air; and also in a dog in which the muscles of the glottis were paralyzed by section of the recurrent nerve. These experiments, the advocates of tracheotomy in epilepsy have found it convenient to ignore.

Again, I would ask, is tracheotomy either a very simple or a very safe operation? I do not think that the results of the operation either for other diseases, or for epilepsy, are very satisfactory. Patients often die from the effects of the operation; exhaustion, erysipelas, diffuse inflammation of the areolar tissue spreading into the mediastinum.

And, lastly, what are the prospects that, the operation having been done, the evil will not recur? This can only be ascertained by that which I cannot recommend, frequent experiments. I would only make this remark; that those who have the courage to try these experiments, and feel themselves justified in so doing, must carefully watch the results, and endeavour to draw a proper distinction between the actual physical effects of the operation, and that mental influence which, as I have shown you in a former part of this lecture, undoubtedly tends to stay the disease, when any new treatment is adopted, or any considerable change takes place in the patient's position and circumstances.

But, gentlemen, let us look on, and watch the result of these operations in the hands of others. No one will be more ready to acknowledge himself in the wrong than I shall be, should it turn out that, in this proposal, an important remedy has been found for a most terrible malady.

In conclusion, I have to express my fears that I have drawn but a sorry picture of the power of our art to deal with this formidable disease. But let not this discourage you; and do not fall into the notion, nor countenance it in any way, that epilepsy is an incurable malady, and that epileptic patients may as well be abandoned to their fate. There is no doubt that some may be completely cured, many very greatly alleviated, and that all should, as far as possible, be the objects of medical scrutiny and care, with at least the object of finding out more and more of the natural history of the malady, and of the *juvantia* as well as the *lædentia;* and with a hope that, at some time or other, a remedy may be vouchsafed to us, or at least that we may gain some insight into the intrinsic nature of this formidable scourge of mankind.—*Med. Times and Gaz.* Aug. 12, 1854.

13. *Chloroform in Hypochondriasis.*—Professor OSBORNE stated at a meeting of the College of Physicians, in Ireland (June 1, 1853), that he had lately, in three cases, opportunities of observing a peculiar effect of chloroform taken into the stomach, in controlling the depressing and saddening feelings belonging to hypochondriasis. Considering that state to be produced by a depraved sensibility of the stomach or colon, and frequently of both, he was led to the internal employment of chloroform, which, being promptly volatilized at the temperature of the stomach, and before being decomposed by the process of digestion, ought to be expected to act as a local anæsthetic, even though the dose should not be sufficient to produce any change in the function of the brain.

The first patient who presented the conditions requisite for this experiment was a married woman, and a mother, aged thirty-three, of a querulous disposition, as was well marked in her countenance, and who had been on a former occasion under his care and that of another practitioner, complaining of a variety of pains in the abdominal region; and she, although relieved, yet persevered in the belief that she still had some internal disease. She now appeared to labour under spinal neuralgia. After this had yielded to the application of nitrate of silver to the spine, and some other remedies, she still continued to feel an indescribable sensation of depression, and of internal annoyance, no longer to be referred to the spinal nerves; no cause for it could be detected. The appetite was good, and the action of the bowels regular. In two days, after taking ten drops of chloroform thrice daily, she *began, for the first time, to acknowledge that she was better*, and in a few days afterwards was free from complaint. The second case was that of a caretaker in the Linenhall, aged twenty-nine. He complained of the deepest dejection of spirits, and of an uncontrollable aversion to make any exertion. His countenance expressed sadness and moroseness. All the functions were in a healthy state, except that the heart's action became tumultuous when excited either by emotion or exercise; but no organic disease could be detected. He stated that he had not been addicted to excesses of any kind, and that there was no cause for his lowness of spirits. He got valerianate of zinc, and also pills to regulate his bowels; but although the heart's action became steadier, yet the depression and inward sensation continued the same. After taking twenty drops of chloroform thrice daily, for two days, *he began to confess*, what he never did before, *that he was better*. His sleep being still unsatisfactory, and disturbed by disagreeable dreams, he was ordered to take forty drops at bedtime. He now stated that he slept with a pleasing dream of seeing his brother, who had gone to America. During the two following nights he took the same dose; and, although his sleep was interrupted by the disturbance attendant on a man in a dying state in the same ward, yet when he did sleep, his dreams were pleasant, being usually that he was enjoying the company of the most agreeable of his friends. He was dismissed with a marked improvement in his countenance, and *acknowledging that he was better*.

The third case was that of a farmer, from the county Wicklow, twenty-eight years of age. In him, the poet's exclamation, *Beati agricolæ!* appeared to be completely reversed. The angles of his mouth were drawn down, his brow generally contracted, and he appeared sometimes as if contemplating suicide; yet he stated that he was in comfortable circumstances, married, and with a family. He complained of an inward sinking and sense of depression, so constant and overpowering, that for some months he could not command himself to make any exertion, and had become unable any longer to attend to his business. His bowels were usually torpid, but although he had repeatedly taken purgative medicines with effect, yet he had obtained no further benefit. A careful examination having been made without detecting any disease, he was ordered to take ten drops of chloroform thrice daily, and two assafetida pills every alternate night. At the end of about four days of gradual improvement, his countenance had become placid. *He confessed that he felt much better;* and in a few days afterwards, feeling a strong desire, and also a capability, of resuming his ordinary avocations, he went home.

These cases are selected as being nearly free from complication. It must, however, be recollected, that there are several other uses to which chloroform may be applied in affections of the stomach and intestinal tube; but this appears

to be one of the greatest value, inasmuch as no other medicine can be named which in this respect seems to come into competition with it. How far the effect is permanent, and capable of completely removing the sensation of hypochondriasis, or in what degree it may require to be resumed or repeated, Dr. Osborne as yet has not been able to determine; neither did he think it necessary, before this Association, to clear himself from the absurdity of bringing it forward as a universal *nepenthes*. Like all other agents affecting the nerves of sensation, its potency must vary much in different individuals, requiring various doses, while in some cases it may fail altogether.

With regard to the mode of administering chloroform internally: as its specific gravity is nearly 1.5, and it is insoluble in water, it must, when swallowed, soon settle at the bottom of the fluids in the stomach; and although it is volatilized, yet being covered, and under pressure, it may remain in contact sufficiently long to irritate the stomach at the part of contact, as was proved to take place in the case of camphor by Orfila. Hence, then, it is desirable that it should be diffused or diluted before it is taken. In aqueous mixtures, even when shaken up, it soon falls so that it cannot be equally measured out, and its pungency is annoying even to the mouth. In gum Arabic mucilage, it soon collects in large globules at the bottom of the bottle, covered with a white powder of arabine which it has precipitated. To obviate this inconvenience it has been proposed to give it suspended in syrup, but to make a syrup of the same specific gravity 1.006 grains of sugar to the ounce of water would be required, while that of the Pharmacopœia contains only 874 grains; besides, chloroform has a heavy sweet taste which renders the addition of syrup peculiarly objectionable.

The menstruum which Dr. Osborne used in the above and other cases, was the decoction of Irish moss (carrageen). With this, chloroform forms a uniform mixture, and in the proportion of ten drops to the ounce, they remain for an indefinite time without separation taking place. The taste of the mixture is sweet, like that of a heavy syrup, to relieve which it may be well to add a few drops of some aromatic or bitter tincture.

Another mode of avoiding the pungency of chloroform, is by giving it in combination with tinctures, as it is soluble in alcohol, and remains dissolved even in proof spirits. The following is a specimen of this kind of formula, and is peculiarly grateful to the taste, and susceptible of various additions and alterations, according to the requirements of individual cases: Chloroform, and tincture of ginger, of each half an ounce; aromatic spirits of ammonia, two drachms. Mix. Twenty-five drops to be taken thrice daily in a wineglassful of milk.—*Dublin Quarterly Journ. Med. Science*, Nov. 1853.

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14. *On the Use of Vegetable and Mineral Acids in the Treatment, Prophylactic and Remedial, of Epidemic Disorders of the Bowels.*—An interesting paper on this subject was read before the Epidemiological Society, July 3, 1854, by J. H. Tucker, Esq.—The author commenced by alluding to the remarkable, but well-established fact, that in 1849 the cider districts of Herefordshire, Somersetshire, and part of Devonshire, were, to a great extent, exempt from the epidemic ravages of cholera, while the disease was raging around. Upon further inquiry, it was ascertained that this exemption was confined a good deal to those individuals who drank cider as a common beverage, and that those who partook of malt liquor occasionally suffered. He also remarked that, in some parts of France and in Normandy, more particularly where cider is the common beverage, cholera is seldom known to exist; and further, that Switzerland was reported to have been free from its visitation.

Having adduced these and other facts in proof of the prophylactic power of cider, the author expressed his opinion that other vegetable acids would be found of service, such as lemon-juice, orange-juice, and sour wines made from grapes, or even from gooseberries. And as it would be found impossible to supply the whole of London with a sufficient quantity of pure cider, Mr. Tucker suggested that *vinegar* might be found a useful substitute in case of another outbreak of cholera, provided that it could be obtained in a state of purity. In confirmation of his view of the sanative and medicinal virtues of vinegar, the author quoted Hippocrates, who (*de natura muliebri*) "employed

white vinegar medicinally"—Plutarch and Livy, who refer to the use of vinegar by Hannibal, in his passage over the Alps, when he is said to have "softened the rocks with fire and vinegar," an operation which the author facetiously regarded as rather metaphorical than chemical, as the vinegar, swallowed by the troops, probably sustained their strength, and thus in effect softened the asperities of their rough way. The author also quoted from Roman history the story that "Scipio Africanus is said to have gained a great battle with a few skins of vinegar," the troops refusing to march until the general had obtained a supply. Cæsar was also reported to mention in his Commentaries the supply of vinegar to the troops; and Mr. Tucker remarked that the drink of the Romans in all their campaigns was vinegar and water, and, sustained by that beverage, they conquered the world. Modern authors (Sir John Pringle, Sir Gilbert Blane, and others) were also quoted in proof of the antiseptic and medicinal qualities of vinegar. The author then proceeded to show that acid drinks were not only preventive, but remedial in epidemic disorders of the bowels. Cases were related, in which not only persons were exempt from attacks of cholera raging around them, who drank large draughts of cider, but a case of severe cholera was also related, which yielded to the diluted juice of sour apples. The efficacy of the *Mineral Acids*, especially the sulphuric, in diarrhœa, and especially in choleraic diarrhœa, was also advocated by reference to numerous facts and authorities. He also referred to some established facts connected with the spread of epidemic dysentery in the army, showing the efficacy of vegetable acids in that disease.

In conclusion, Mr. Tucker suggested a necessary caution relative to the use of the wretched and unwholesome substitute for vinegar commonly sold in the London shops.

The discussion which followed the reading of the paper, elicited many facts in confirmation of the author's views; and, as to the efficacy of sulphuric acid largely diluted with water, in choleraic diarrhœa, there was not a dissentient voice.—*Lancet*, July 15.

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15. *Sulphuric Acid in the Treatment of Cholera and Choleraic Diarrhœa.*—Dr. H. W. Fuller, Assistant Physician to St. George's Hospital gives (*Med. Times and Gaz.* Aug. 12, 1854) the following statement relative to the efficiency of sulphuric acid in the treatment of cholera and choleraic diarrhœa, deduced not only from his own experience, but also from the concurrent testimony of a number of practitioners.

"Firstly. As to the form in which the medicine should be administered. One ounce of the dilute sulphuric acid of the Pharmacopœia should be added to eleven ounces of water, and of this mixture three tablespoonsful should be given as a dose. The acid is very grateful to the palate, and to the stomach; so I rarely mix it with syrup or any flavourings, which are apt to nauseate, if not to interfere with the action of the remedy. Sometimes, however, I add a drachm or half a drachm of chloric ether to every alternate dose of the medicine, and occasionally, at the outset of the attack, administer two grains of opium in a pill, combined, it may be, with five grains of calomel. If the first stage of the disease is passed, I never administer opium.

"Secondly. In ordinary cases of choleraic diarrhœa, implying by that term cases in which there is thin watery purging, and possibly vomiting, with faintness and coldness, and clamminess of the surface, a pale moist tongue, crampy pains in the belly, and possibly also cramp in the extremities, three or four doses of the acid mixture, taken at intervals of half an hour, will generally suffice to effect a cure. The vomiting and purging will be stayed, the cramps will subside, and heat will return to the extremities. Little or no after treatment is usually required.

"Thirdly. In confirmed cases of cholera, characterized by the symptoms already enumerated, but accompanied by a feeble or imperceptible pulse, collapse and blueness, or lividity of the countenance and extremities, a dose of the acid mixture should be administered every twenty minutes, until warmth returns to the extremities, and colour to the lips. The addition of chloric ether to each dose of the mixture is extremely serviceable, if tolerated by the stomach. As the

symptoms subside, the medicine should be exhibited at longer and longer intervals. In all cases, whether of confirmed cholera or of choleraic diarrhœa, a mustard poultice should be applied to the pit of the stomach, and frictions to the extremities, and a dose of the mixture should be given *immediately* after each act of vomiting.

"Fourthly. In cases of collapse, it is advisable to commence the treatment by a brisk mustard emetic. Through its agency, not only is the stomach emptied, and thus put in a state to be acted on by the medicine, but the dormant energies of the system are aroused.

"Fifthly. In no single instance, either in my own practice or in that of others, have I known the slightest ill effect from these full doses of the acid; while experience has proved that in very severe cases the medicine administered in smaller doses is inadequate to excite reaction and save life. Within my own experience, six doses have always proved sufficient to effect a cure. and I should not feel disposed to persevere in administering the medicine beyond the eighth dose.

"Sixthly. The cases in which the acid proves most efficacious are cases of true cholera and choleraic diarrhœa, marked by the symptoms already alluded to. In the majority of such cases it acts like a charm. In ordinary bilious diarrhœa, accompanied by a foul tongue and not by coldness of the surface, the ordinary remedies for diarrhœa have appeared to me more efficient. Even in these cases, however, no harm results from the administration of the acid, and much good is often effected; so that, during the continuance of the choleraic influence, every person may be safely recommended to keep a bottle of the acid mixture in his house for immediate use, until the arrival of a medical man.

"If the public were made aware of the fact, and were to act upon it, I am satisfied that the mortality from cholera would soon be reduced within very moderate limits. This I state as the result of actual experience in above a thousand cases, many of which have occurred in my own practice."

16. *Strychnine in Cholera.*—Prof. W. Fraser, of McGill College, Montreal, has been led, on physiological grounds, to try the effect, in cholera, of minute doses of strychnine as a *general stimulant*, for sustaining the vital powers on the eve of their failure, and for restoring them to functional action when that is all but suspended by the supervention of collapse.

The indications which have guided him in the administration of strychnine, are the following:—

"In all cases of real Asiatic cholera (tested by the evacuation of rice-water stools), even before the supervention of collapse, the coming failure of the circulation is indicated by less or more irregularity or fluttering in the rhythmic action of the pulse. When the case is seen at this stage, the strychnine should be at once commenced simultaneously with means for arresting and correcting the discharges. Given at this early period, it will rarely fail to arrest the patient's descent into collapse. From six to twelve doses will usually cause the pulse to become firmer and more regular, and the anemic appearance due to the serous discharges from the alimentary canal, to be replaced by an active capillary circulation. In those cases, on the other hand, that are in a state of collapse when first seen, the principal indications are to bring on reaction, and to arrest the discharges, when these still continue. All who have seen much of cholera, must be but too familiar with the frequent failure of ordinary stimulants in bringing on reaction; hence the necessity of employing such an extraordinary and powerful agent as strychnine, which, according to my experience, will often, but not always, succeed in doing so, in cases where all other stimulants would fail. In some desperate cases, other stimulants, as tea, camphor, brandy, or champagne, may be advantageously given, when the stomach will bear them concurrently with strychnine; they will, however, be very commonly rejected, while strychnine alone is retained."

The dose he has found "most suitable is the one-forty-eighth part of a grain, dissolved in acetic acid and alcohol, and repeated every quarter of an hour, or every five or ten minutes, in very severe cases, till the pulse, if it has been fluttering, as in incipient collapse, becomes steady, or till reaction is established

in cases of collapse—when these objects are accomplished, the interval between the doses ought to be lengthened—and should the specific effect of the medicine on the nervous system be produced, then its exhibition should be suspended till these disappear, when it may be again administered, if deemed requisite, at longer intervals. The medicine should be thus continued till the circulation is fully and firmly established, and the patient is fairly beyond the risk of collapse. During the administration of this dangerous remedy, the patient should be frequently seen by the physician, in order to watch its effects, and direct its discontinuance, should its effects on the nervous system manifest themselves."

The number of cases in which Dr. F. administered the quinia is twenty-two. A tabular view of these are given.

This table shows "that the number of deaths in the twenty-two cases was five, or about twenty-two and a half per cent. It will also be noticed in regard to the five deaths, that in four of them reaction came on, and they died, not of cholera, or at least, not in the collapsed state of cholera, but of the secondary fever which so often follows it. The only one that did die in collapse was William Anderson, and he (though several hours in the hospital) was but one hour under the strychnine treatment, during which Mr. Loverin, the student who watched him, tells me he took four doses only, so that in his case it can hardly be said to have had a fair trial. I conceive it is, therefore, deserving of special notice, that in all the cases (and some of them were of the very worst description), except in Anderson's, the strychnine was successful in accomplishing the purpose for which it was prescribed, namely, bringing on reaction, which tends to prove its superiority over all other stimulants hitherto employed in this disease."—*Medical Chronicle*, Sept. 1854.

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17. *Subnitrate of Bismuth administered in Enemata for the Cure of Diarrhœa and Inflammation of the Colon.*—Diarrhœa, attended with glairy and bloody evacuations, has been prevalent of late in France. Two infants, both at the breast, have been successfully treated by M. Trousseau, by the subnitrate of bismuth, given in enema. One of them had had cholera, characterized by vomiting, diarrhœa, coldness, and well-marked emaciation. Opium, nitrate of silver, and other remedies were given, by which the sickness was arrested, but the diarrhœa continued. The other child had not had vomiting, but there was a glairy diarrhœa, showing that the disease was limited to the large intestine. M. Trousseau prescribed a lavement according to the following formula:—

Subnitrate of bismuth two scruples. Thick linseed-tea sufficient. The mixture to have the consistence of soup.

The diarrhœa ceased immediately upon the administration of the lavement.

It is known that M. Monneret gives in similar cases to infants as much as two or three ounces of the subnitrate of bismuth daily. M. Trousseau rarely goes beyond four scruples; very often he is content with half that quantity administered in water, or in the form of pastilles. Dr. Lassegue, chief of the clinique of the Hôtel-Dieu, first entertained the idea of employing it as a lavement in cases of inflammation of the colon, from having witnessed its beneficial effects in certain diseases of the skin. Some of the subnitrate mixed with albumen or thick linseed-tea, spread upon a poultice, rendered humid by the application of glycerin, was brought into contact with the affected parts. M. Lassegue carried out a somewhat similar plan in the large intestine (?), and obtained results which deserve to be recorded; the more so, because the remedy, being harmless, may be applied without fear to all similar cases.—*Journ. de Méd. et de Chirurg. Prat.*

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18. *Tapeworm unsuccessfully treated by the Extract of Male Fern and Kousso.*—Dr. George Patterson relates (*Monthly Journ. Med. Sci.* July, 1854) three cases of tapeworm recently treated by him, and derives the following conclusions from them:—

1. It would appear from the above cases that the kousso and ethereal extract of male fern are, like all other remedies that have hitherto been prepared for tapeworm, liable to, at least, occasional failure. At the same time it is certain, from the effects that followed upon their administration in these instances, that

they are remedies of great power, very obnoxious to the parasite, and, therefore, valuable additions to our means of treatment.

2. I should be disposed to believe, from what was observed in these cases, that the same remedy is not suited alike to every case; but that in one subject the kousso, in another the male fern, and in another the turpentine, will be found to be the most appropriate remedy. Thus, in the second case, neither the male fern nor the kousso brought off any of the worm, while turpentine invariably did so. In the first, on the contrary, turpentine and castor-oil were repeatedly given before the male fern, without having the least effect. Again, we sometimes find the worm not only detached and dislodged, but carried downwards and expelled by the male fern and kousso, unaided by any purgative; while at other times they require to be followed by the administration of a purgative dose. As to the comparative energy of the kousso and male fern, it would appear from the above cases (in which both were successively given to the same person) to be nearly equal, and in the phenomena attendant upon their operation both remedies are very much alike.

3. The first of the cases I have narrated is in many respects a remarkable one. It furnishes a striking portraiture of the symptoms of the disease in a very aggravated form; and the immense number of joints that were discharged at various times, both singly and in pieces of several feet in length, would seem to indicate either that there must have been more than one of the parasites in the man's body, or else that it is capable of reproduction, and of attaining its former growth in an incredibly short space of time after large portions of it have been detached.

4. The same case affords an instance of the worm finding its way into the stomach, portions of it being stated to have been brought up by vomiting in round balls. This is a more common occurrence with other kinds of intestinal worms. Thus, I have met with several instances, both in children and adults, where lumbrici have escaped by the mouth or nose. In one case of an hysterical and hypochondriacal female, the circumstance of one of these worms having escaped by the mouth, produced an impression that she still had one in the windpipe. The sensation, which was partly hysterical, and partly perhaps due to an enlargement of the thyroid body, led to continual efforts to bring up the worm and dislodge it with the finger, which necessarily produced increased local irritation, and the failure of which aggravated her despondency and low spirits. She vainly resorted to several medical men, including myself, for relief—neither reasoning nor such remedial measures as the nature of the case suggested having succeeded in overcoming the delusion.

5. In the second case, we have an instance of the coexistence of another kind of worm with the tapeworm. This is worthy of notice, principally as showing (what has been pointed out by Dr. Watson and others) that the term "solitaire," which some French writers have applied to this worm as a distinguishing characteristic, is not correct.

6. Besides the above three cases, falling under my own notice in the course of a few months, I have heard of another instance, under the care of one of the medical gentlemen of this town, in which the kousso brought off several yards of a tapeworm, but equally failed to eradicate the disease.

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19. *New Remedy for Tapeworm.*—C. Mackinnon, Esq. gives (*Indian Annals of Med. Sci.* Oct. 1853) the result of his administration of a remedy in tapeworm, new, probably, in European practice. This is the "Kameyla," a drug procurable in all the bazaars of Northern India, used as a dye by the natives, and in common use as a vermifuge for dogs. "My attention," he says, "was first called to it by a gunner of the brigade, affected with tapeworm, in whom both turpentine and kousso had failed to expel the worm. He stated that a companion of his, affected with tapeworm, had taken the remedy with success. I immediately sent for some, and without any previous preparation of the patient gave him three drachms. He was a large, powerful man, and this producing no effect, in four hours afterwards the same dose was repeated. It now operated very freely and frequently, and with the fourth stool a large tapeworm six yards long was passed.

This result was so satisfactory that I have continued to employ the remedy whenever a case presented itself; and I have now given it in sixteen different cases, and in all without a failure. As far as my experience goes, I have found it a better and more certain remedy than either turpentine or kousso, and much less disagreeable to take than either of these remedies.

In none of my cases, subsequent to the first, did I ever exceed, for a single dose, three drachms. This usually purges from five to seven times, and the worm is usually expelled dead in the fourth or fifth stool.

In two of the latter cases, in which I administered it in hospital, both patients recently recovered from fever and still weak, the dose of three drachms purged very violently from a dozen to fourteen times. In three subsequent cases, I reduced the dose to ʒiss, and no action on the bowels succeeding it, I gave, in six hours afterwards, ℥ss of castor-oil. This acted four or five times, and in each case the worm was passed dead. In one case, in which ʒiij were given, though the medicine acted as usual, the worm was not passed until next day; and in another, in which the same dose was administered, followed by the usual operation, only a portion of the worm two feet long was passed. In this case, on the succeeding day, a dose of ʒiss was given, followed in six hours by ℥ss of castor-oil. No portion of the worm being passed in the evacuation following the castor-oil, on the succeeding day, ʒi given in the morning, again in the middle of the day, and again at night. Four or five motions followed the medicine given in this manner, and a worm eight feet long was passed dead.

In almost every case, the long slender neck of the worm appeared in the motion.

To a native child of five years of age, I gave a dose of forty grains, and a tapeworm as duly expelled. The drug usually purges speedily. In about half the cases, some degree of nausea and slight griping were experienced; in the remaining half no inconvenience whatever was sustained, some of the patients declaring it to be the easiest purge they had ever taken in their lives.

I have not thought it necessary to subject the patients to any previous preparation; but it is not unlikely that if a laxative were taken the day before, so as to sweep out the intestines, and the patient directed to fast, that a lesser dose might then act on the worm, deprived as it would then be of a protecting medium of feculent matter. I have usually given the remedy in ℥i of cinnamon water.

I do not advance the remedy as a specific against the disease; all remedies fail occasionally, and I suppose this one will also; but it has hitherto proved in my hands the most successful and most certain remedy in tænia that I have yet used. In at least three cases, in which turpentine and kousso both failed, I have succeeded with the "kameyla."

The following I should sum up as the results of my experience:—

1. That "kameyla" is a safe and efficient remedy for tapeworm, and more certain than either turpentine or kousso.

2. That to a strong European ʒiij may be safely given as a dose.

3. To a person of feeble habit, or to a female, ʒiss, followed, if necessary, by ℥ss of castor-oil, is a sufficient dose.

The "kameyla," as sold in the bazaars, is a powder of a brownish-red colour. It is gathered from a shrub or tree, the rottlera tinctoria of the natural order euphorbiaceæ, which grows abundantly along the lower ranges of the Himalayas. It is a powder which is attached to the fruit-capsule, and when the fruit is ripe, it is brushed off and collected. It is used as a dye, imparting a bright orange colour to silk, and is sold in the drug mart of Umritsur, at twelve rs., a maund for the first quality, and ten rupees for the second.[1]

The drug is used by native practitioners made into an ointment, as an external application in itch and other skin disorders, and its use as a vermifuge is also known to them. It is very probable that the attention of European practitioners has not been previously called to it as a vermifuge, from the smaller doses in which native practitioners administer it, and which must have rendered its use often unsuccessful."

[1] This is at the rate of 2 lbs. for sixpence.

20. *Cases of Gangrene of the Lung successfully treated by Inhalations of Turpentine.*—By Prof. Skoda, of Vienna. These cases are three in number, and Dr. Skoda has made use, in each of them, of the vapours of turpentine, along with sulphate of quinia. The first case is that of a domestic servant, who was affected with gangrene limited to the upper lobe of the right lung. After six weeks of this treatment, all the physical signs had disappeared, and vesicular respiration free from râles was audible. The patient was seen three months later, and remained perfectly well. The subject of the second case was an innkeeper, of strong constitution, somewhat advanced in life, in whom a gangrenous cavity existed in the lower lobe of the right lung. The disease dated from the 11th of March, 1852. On the 21st of March, inhalations of turpentine were commenced. The patient employed them without any repugnance for five or ten minutes every two hours, and in addition took every two hours a grain of sulphate of quinia. In three weeks the expectoration, which had been very abundant, was reduced to a sixth. In six weeks the patient could leave his bed, but the expectoration continued sanious and of a fetid odour. His strength returned, his appetite improved, and in the month of June his general appearance was satisfactory, although cough and expectoration, occasionally fetid, continued. The right side of the thorax was painful, the breathing difficult, but dulness on percussion had entirely disappeared; the respiration was indeterminate, and accompanied by feeble sibilant râles. The patient went to the country and continued the turpentine inhalations twice a day till the month of July, by which time the cough and expectoration had entirely disappeared. In December, he had perfectly regained his flesh and strength, was free from cough or pain, no retraction of the thorax had taken place, and vesicular breathing was everywhere audible. The third case was that of a butcher, suffering from gangrene of the lower lobe of the left lung. He turned ill towards the end of May, 1852. On the 4th of June, he began to employ turpentine inhalations. Within eight days the fetid expectoration had ceased, and the patient believing himself recovered, as the pain and oppression had diminished, ceased the use of the turpentine, as it was disagreeable to him. On the 19th of June, a violent shivering came on, accompanied by cough, dyspnœa, and fetid expectoration. The inhalations were resumed, and gradual improvement took place. They were, however, again suspended, but a fresh exacerbation occurred. His appetite was gone, and his emaciation and weakness were such that, about the end of July, he could not leave his bed. Fowler's solution was then had recourse to, and under its influence, as well as that of the inhalations, the patient recovered so far that he could leave his bed and go to the country. By the middle of October, he had regained flesh and a considerable degree of strength; but a feeling of pain and oppression remained below the scapula and in the left side of the thorax, while there was a little cough with slight fetid expectoration from time to time. Vesicular respiration was audible to the base of the lung, but dulness continued to occupy an extent equal to the size of a pleximeter, in the neighbourhood of the apex of the heart; at this level faint râles were audible. The turpentine inhalations were ordered to be continued. At the end of January the patient believed himself cured; for ten days he had had no chest symptom, when, on the 10th of February, after a slight fit of coughing, he expectorated two ounces of blood, and after it some fetid pus. There were no signs of a new infiltration. The blood probably came from the gangrenous cavity, the walls of which had become callous.—*Zeitschrift für K.K. Geselischaft der Aertze zu Wien, in L'Union Médicale*, Feb. 16, 1854.

[The inhalations are practised by pouring a little oil of turpentine upon boiling water, and respiring the vapours.]—*Monthly Journ. Med. Sci.* July, 1854.

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21. *Prurigo Formicans of the Vulva and Anus.*—M. Richard recommends (*Revue de Thérapeutique Médico Chirurgicale*) the following preparation as extremely useful in the cure of prurigo formicans and various kinds of eruption: Take of sulphate of zinc and sulphate of alumine equal parts; reduce these salts to a coarse powder, and put them upon an earthen dish coated with oxide of lead; place the dish on a gentle fire, and leave it until the mixture no longer emits air-bubbles and is turned into a stony consistence. Take the dish from

the fire, pound the petrous substance into a fine powder, and throw about half an ounce of it, in divided parts, at about a minute's interval (to avoid too powerful an effervescence), into a quart of boiling water; then filter through paper, and keep the lotion for use. Every morning apply some of this solution with a fine sponge on the affected part; then, if it be the anal orifice which is suffering from prurigo, soak a piece of linen folded square (a little better than an inch to each side), and introduce one of the angles into the orifice. The mode of dressing is the same for the vulva. This lotion is also extremely useful for various kinds of eruption, but in the latter cases internal medicines should also be taken; these should principally consist of alkalies, bitters, and purgatives.

22. *Successful Mode of Treating Mercurial Salivation.*—Dr. Norman Chevers states (*Indian Annals of Medical Science*, April 1854), that he has met with uniform and apparently certain success from the use of iodine gargles in the worst cases of mercurial ptyalism. He has found that a gargle containing from two to five drachms of the compound tincture of iodine to eight ounces of water, exerts an absolutely prophylactic or curative influence. He states also, that Mr. Burgess has applied the pure tincture to the whole interior of the mouth, in cases of severe mercurial salivation, with speedy and perfect success.

Dr. Chevers, among other cases, quotes the following, in illustration of the benefit of this treatment:—

"*Cure of Ordinary Cases of Salivation.*—In February 1852, I attended an officer, ætat 48, in an attack of cholera, which was then raging epidemically in Chittagong. The disease was generally attended with extreme danger, and this case was one of remarkable severity. During the first sixteen hours, I administered seventy-five grains of calomel. On the third day the sputa became tinged with blood, the gums were swollen and tender, and the spaces between the teeth were filled with coagula. A gargle, containing two drachms of compound tincture of iodine, to eight ounces of water, removed all traces of salivation so effectually within about two days, that my patient, although a very intelligent man, and a rather active dabbler in physic, never appeared to be aware that he had been subjected to mercurial treatment.

"Early in last year, I was called to attend a lady, about thirty-four years of age, who had been suddenly attacked with an excruciating pain across the umbilical region, which appeared to be associated with a sudden check to the catamenial function, resulting from exposure to a draught while very thinly clad. The symptoms were extremely urgent, and a dose of ten grains of calomel was among the first remedies employed. Relief was obtained almost at once; but on the second day, the tongue was found swollen, and clots occupied the interspaces of the teeth, but little uneasiness was complained of. The iodine gargle was employed with such rapid success, that the patient scarcely referred a second time to the condition of her mouth.

"*Employment of Iodine as a Prophylactic.*—Of late, I have been in the habit of beginning to employ the gargle in all cases where the quantity of mercury given has been such as to render the occurrence of salivation probable. Judging from a confessedly very limited experience of this measure, I apprehend that its early employment will anticipate the occurrence of salivation in all cases where the constitution is good, and there is little or no visceral disease; that, even under the worst circumstances, it will greatly limit the severity of the action; and that, for the most part, the original disease, on account of which mercury was administered, will have its decline rather favoured than otherwise by the absorption of iodine from the mucous membrane of the mouth. A certain degree of doubt will, of course, attend nearly all details of prophylactic treatment, but I think that the following cases may be regarded as encouraging.

"Early in the last rains, I was requested to visit a medical officer suffering extremely from an attack of ileus, which I attributed to the sudden outflow of a quantity of a highly vitiated bile, acting as an almost corrosive irritant upon the mucous membrane of the small intestines. When called to him, I found him greatly weakened by intense pain and obstinate vomiting, and by oozing from the bites of several leeches which he had himself applied to his abdomen.

He had already taken three or more five-grain doses of calomel, which, however, had not acted upon the bowels. I administered a ten-grain dose almost immediately; and, the disease remaining obstinate, a scruple dose was recommended by Dr. Miller, who met me in consultation, and was given on the following day. The iodine gargle was employed early; and, although it was nearly certain that a large proportion of from forty-five to fifty grains of calomel must have fully entered the system, ptyalism did not occur.

"In October last, I attended the children of an European for mumps, which was then rather prevalent in Howrah and its vicinity. All these children had suffered from hooping-cough during the preceding winter. The elder boys did well, but the two youngest, aged respectively about two and four years, were suddenly attacked with laryngitis as the swelling of the parotids began to decrease. Although very actively treated, the younger infant died in little more than twenty-four hours from the onset of the laryngeal symptoms. I found the larynx and trachea absolutely occluded by an exceedingly tenacious croupy deposit. The other child's symptoms were equally violent, but he recovered under very severe treatment, a part of which consisted in the administration of fifteen grains of calomel within as many hours. In a day or two, one or two aphthæ appeared on the tongue, yet it could scarcely be said that salivation was present. The gargle was used freely and no further annoyance was experienced, although the tongue has ever since presented that patchy appearance not unfrequently noticed among delicate children in India."

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23. *Diabetes Mellitus.*—Probably one of the most elaborate papers ever written on this disease has just been published by Dr. Th. Von Dursch, of Manheim. It is founded upon two very interesting cases of diabetes, of which he has given very careful clinical reports, and also accounts of the pathological lesions found after death. With indefatigable perseverance and most praiseworthy zeal, he carefully ascertained, every day for several months, the nature and amount of the food and drink taken by his patients; the amount of fluid contained in the aliment; the quantity of urine excreted, its specific gravity, and the amount of sugar it contained; the number of the stools, the proportion of their watery constituents; the amount of water exhaled by the lungs and transpired by the skin, &c. The results of these most laborious and minute investigations he has condensed into two large synoptical tables, which are appended to the memoir. In one of these, full particulars are given of the effects of different kinds of diet upon the total amount, specific gravity, and saccharine constituents of the urine. As far as our limited space permits, we shall now glance at the general results thus tabulated. *First,* when the patient was put upon a mixed diet for forty days; *second,* when a farinaceous diet alone was allowed during eight days; and, *thirdly,* when animal food was given during a period of five days.

1. *Effects of mixed diet on*

(*a*). *The specific gravity.*—The average density of the urine, while this regimen was adhered to, was 1037.8; it was higher in the mornings and evenings (1038) than during the day (1036).

(*b*). (*The amount of the urine.*—The daily average was 5234 cubic centimetres. The quantity voided was greater in the morning (1971 c.c.) and evening (1831 c.c.) than during the day (1430).

(*c*). *The percentage of sugar.*—The average amount of saccharine matter was 9.134 in the 100 grammes. The percentage was lower in the morning and mid-day urine (8.9) than in that passed at night (9.4).

(*d*). *The total amount of sugar.*—The average daily amount of sugar excreted during the whole period was 477.7 grammes; the lowest was 350, and the highest 615 grammes.

2. *Effects of farinaceous diet on*

(*a*). *The specific gravity.*—This continued nearly the same as with mixed diet; the average was 1037.6. With this regimen, also, it was lower during the day than at night or morning.

(*b*). *The amount of the urine.*—This was increased to 5604 cubic centimetres

as its daily average. It was greater in the morning (2165 c.c.) than during the day (1737 c.c.) or at night (1701).

(*c*). *The percentage of sugar.*—This continued nearly unchanged, being on an average 9.39 in 100 grammes.

(*d*). *The total amount of sugar.*—In this a considerable increase was visible, while the farinaceous diet was continued. The average quantity of sugar daily excreted amounted to 526.4 grammes; and the urine in the morning contained more (201 gr.) than at noon (159g.), or at night (165 gr).

3. *The effect of animal diet on*

(*a*). *The specific gravity.*—It remained nearly unaltered by this regimen, as happened with both the other diets. Its average was 1037.2; and it was lower in the morning (1036) than at other times (1037).

(*b*). *The amount of the urine.*—This was considerably diminished, the average quantity *per diem* being 4588 cubic centimetres. The average amount was much greater in the morning (1816 c.c.) than during the day (1324 c.c.) or at night (1448 c.c.).

(*c*). *The percentage of sugar.*—This was also lessened, being on an average 8.232 in 100 grammes.

(*d*). *The total amount of sugar.*—Here, likewise, a striking diminution was manifest. The average quantity daily excreted was 379.8 grammes; this was greatest in the morning (139 grs.), and less during the day (114 grs.) than at night (126 grs.).

Dr. Von Dursch discusses several of the questions relative to diabetes, and brings to bear upon them the weight of his experience and careful observations. As regards the disputed point, whether the quantity of the urine voided in this disease surpasses the amount of the fluids absorbed, he thinks that the question has not been properly considered, and that we ought to compare the amount of water in the urine, &c., with that contained in the food and drink taken. During his investigations, he ascertained the amount of the cutaneous transpiration and pulmonary exhalation by frequently weighing his patient; and he also carefully noted the quantity of water contained in the feces. He has succeeded thereby in satisfying himself that the water given off by the patient equals exactly the amount of the water absorbed by him.

In conclusion, the author believes, from all his researches, that diabetes principally depends on the sugar normally existing in the blood being undestroyed and unappropriated; and he is of opinion that all kinds of food are capable of producing sugar.—*Monthly Journ. Med. Sci.*, from *Henlè und Pfeuffer's Zeitsch. für Rationelle Medicin.* 1853. Bd. iv. Hft. i.

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24. *Betz on the Thyroid Asthma of Young Children.*—Medical men are divided on the subject of thyroid and thymic asthma; some acknowledging, and others altogether denying their existence. Alois Bednar, for example, in his recent work (*Krankheiten der Neugeborenen*, pt. 3, page 77), does not believe in the existence of asthma resulting from hypertrophy of the thyroid gland alone. He relates ten cases of thyroid enlargement, nine of which were attended with asthmatic symptoms; but in all of these nine cases there existed other pathological affections of the air-passages, sufficient to account for the dyspnœa. He alleges that the thyroid gland may be enlarged without the respiration being affected, but confesses that most cases of hypertrophy are attended by a difficulty of breathing; and he considers the dyspnœa so induced as not asthmatic in its character. Friedrich Betz, of Heilbron, has published an interesting paper on the subject, in which he regards the impediment to the entrance of air, in cases of thyroid hypertrophy, as existing neither at the upper part of the trachea nor in the larynx, but higher up, at the entrance to the air-passage, where the enlarged gland projects into the floor of the mouth. The thyroid body has this peculiarity of conformation in new-born infants, so that both its lateral lobes extend backwards, between the pharynx and the vertebral column; and, according to Albers (*Constatt's Jahresberichi*, 1848), the lobes may even become blended together, so as to form a fleshy ring round the larynx and top of the pharynx.

When, therefore, its extremities become swollen, they constrict the parts inclosed within them.

This affection is easily recognized by swelling of the upper part of the neck, and sensible enlargement of the thyroid body. Expiration and inspiration are both stridulous; the sounds continue without intermission, only remitting during sleep. The mouth is filled with frothy saliva; the hands, feet, lips, and face, are cold and bluish; there is no fever, there is an occasional paroxysm of coughing, and the sleep is short and broken. Suckling is impossible, for the attempts to swallow fluids increase the suffocative dyspnœa. In severe cases, death occurs after from one to four days; in those which are milder, the swelling gradually subsides, and with it the dysphagia and dyspnœa. It assists our diagnosis to remember that this disease is limited, the first eight to fourteen days after birth.—*Monthly Journ. Med. Sci.* July, 1854, from *Jour. für Kinderkrankh*, Sept. and Oct. 1853.

SURGICAL PATHOLOGY AND THERAPEUTICS, AND OPERATIVE SURGERY.

25. *Modern Philosophy of Cancer.*—Robert Druitt, Esq., in an interesting paper read before the Medical Society of London (*Assoc. Med. Journ.* Jan. 13, 1854), calls attention to the necessity of reform of the classification of cancer, and of the diseases allied to it. These diseases have been classified on three principles: 1. The outward resemblance of morbid growths to natural structures, or to common objects in nature; 2. On the vital qualities of morbid growths—their effects on the health, their curability or fatality; and 3. Physical structure. Mr. Druitt advocates the last base of classification. He thus defines cancer:—

"Cancer is a disease of the blood, manifested by the evolution, in some part of the animal economy, of a structure called the cancer nucleus. This is a bright, oval, well-defined plump body, averaging a little less than one-two-thousandth of an inch in its long diameter, and containing one or more large and distinct nucleoli. In soft, *intense*, rapidly growing cancerous tumours, these nuclei may be free, floating in a liquid, or may be imbedded in a soft, transparent amorphous substance; but usually, this amorphous substance is gathered about the nucleus in a more or less definite spherical mass, called a cell. One cell may inclose one or more nuclei. It becomes much more visible by the addition of water, but vanishes on the addition of acetic acid, which renders the nucleus more bright and distinct. Taken together, the characters of the cancer nucleated cell are, the large size of the nucleus, and of its nucleoli; and the fact that the cells, if abundant and adhering, and ill-developed, are heaped together amorphously; that, if few and well-developed, they have no adhesions to the tissues around them, but readily exude from a cut surface in a milky liquid, called the cancerous juice. This juice in general mixes readily with water; and the cancer cells being, like the similar bodies in milk and pus, not adherent to each other, are equally diffused throughout the liquid, and do not usually clot together as some other cells do.

But, since cell-structures, as they are called, have, besides the cells, some intercellular matter, so in cancer, besides the cancer cell, there is usually some intercell, the varieties of which occasion the varieties of the species. Cancer cells abundantly heaped together, with liquid, or with scanty and delicate filamentous tissue, constitute soft cancer, which is the most intense and rapidly growing example of the species, and which is usually described as the medullary or encephaloid. Scantier growth of cells, more closely packed together in the meshes of fibrous tissue, constitutes the hard, better known as the scirrhous cancer. Immense development of loops of bloodvessels, accompanying superficial cancer, causes it to be called *villous*. When, together with the cancer cells, there is development of a peculiar jelly-like matter, or of a dense imper-

fect bony matter, the cancer is called colloid or osteoid respectively. Other varieties depend on other incidental circumstances; thus infiltration with black pigment gives rise to what is usually called melanic or melanotic, but which might as well be called, in plain English, black cancer. Fatty degeneration occasions the appearance called reticular by Müller, and phymatoid by Lebert. Infiltration with blood produces the well-known fungus hæmatodes.

Whilst alluding to varieties of cancer, I may well take the term *melanosis* as an example of the mischief and confusion arising from the want of definite principles of nomenclature. I might defy any person, reading certain books and journals for the last twenty years, to form a clear idea of what is meant by melanosis. By some persons, the term is used as if synonymous with cancer; by others, not. It is a fair instance of a name which is popular because founded on a character which is striking, but erroneous; because founded on a character which is accidental and variable. In man, black pigment is found in those cutaneous growths commonly called moles; it is also found in some specimens of cancer. In the horse (especially in old grays), deposits of pigment are very commonly associated with fibrous or fibro-plastic growths, which become diffused over the body, just as black cancer in man, and which may soften and ulcerate, or may cause death by accumulating and interfering with the functions of various organs; or, on the contrary, may remain harmless, and allow the animal to reach a good old age. Whether any of the black tumours in man, which may be *diffused* over the body, are of this sort, I know not. But, to return to the subject of classification, the presence of black pigment is no proof that a tumour is a cancer, or that it is not; and it is a caricature of pathology to see, on one shelf in a museum, black cancer from man, and black fibrous tumours from the horse, all arranged under the common head 'Melanosis.'

To return to the subject of cancer. When we speak of *varieties*, we imply the existence of a definite form, the presence of which determines the species; and we believe that the presence of cancer is determined by the existence of the cancer nucleus or cell. Where these exist, there is cancer, no matter with what variety of accompanying circumstances. On the contrary, where there is no cancer nucleus or cell, there, we hold, is no cancer, no matter what else there may be as to structure, and no matter whether the morbid structure which does exist be, or be not, quite as destructive as cancer.

I know very well, however, that some of my professional friends raise an objection to the anatomical or structural basis of classification, on the ground that the anatomical element of cancer, the cell, is not a sufficiently positive quality; or that cancers may exist in which no cancer cell can be detected.

To this, the reply is simple and logical. If, in any growth, after thorough examination by competent persons, no cancer cell or nucleus can be detected, that is evidently not a cancer. Let its anatomical elements be figured and described, and let its clinical history be drawn up for comparison with similar specimens which may hereafter be met with, and let some new and positive name be given to it; but if in any disease, no matter what it is, and how it proves fatal, nothing is found which can be identified with the cancer nucleus, that disease ought to be called by some other name than cancer.

Again, in the search for this structure, there are some sources of error and difficulty which require to be well understood, and which I will describe, as I have myself met with them.

In the first place, the observer, who is studying the cancer structure, must be prepared to meet—just as he must in studying any other series of natural objects—with imperfect, immature, and ill-developed specimens. This, as Lebert observes, will most probably be the case in the examination of rapidly growing and exuberant soft cancers—the fungus hæmatodes or medullaris of authors. Here the nuclei in large masses may be small, and the nucleoli ill marked; the cell substance being hardly defined.

Again, in other specimens, there may be difficulty arising from a directly opposite source, namely, degeneration; for the cells constituting portions of a tumour may be dried up and shrivelled, or filled with oil-globules, or broken down and mixed with large granular cells and crystals of fatty matter.

Again, in every cancerous tumour, the whole mass is not cancer; and it is perfectly possible that portions of a specimen may consist of gelatinous stuff, imperfectly organized areolar tissue, exhibiting fibrillary matter, intermixed with the elongated, caudate, fusiform, and bifid nuclei and cells which may be found in almost every growth; or there may be a confusion, arising from the remains of the structures into which the cancer is deposited.

I mention these possible sources of error, with the further observation, that they are met with equally in all other branches of natural science. Ill-developed, or withered, or monstrous specimens may at times perplex the botanist; but practice, and the art of distinguishing essential characters which it teaches, soon overcome the difficulty. In fact, if any one denies the existence of an anatomical characteristic in cancer, we may ask how he can account for the fact that such men as Hughes Bennett, Paget, Lebert, Hannover, and a host of others, working independently, and far apart, can find, describe, and figure such a structure, and agree with each other in so doing? From my own observations, I believe that any one, who will take the trouble of procuring and examining specimens, and who guards against the possible sources of error which I have pointed out, will soon be convinced of the existence of the cancer cell. Then, to repeat what I have said before, under whatever circumstances this cancer structure may be found, whether it cause hypertrophy or atrophy of surrounding parts; whether it be melanic in colour, or xanthous; whether universally diffused and quickly fatal, or whether it lingers on in one organ, and allows the patient to die of some other disease—there is cancer, with its one specific determining sign.

Now, this mode of treating the subject is simple and definite, and allows statistics a sure basis to rest upon; and it allows morbid products to be accurately defined by their physical forms at the various epochs of their development; and thus the vital qualities and clinical history, the effects of causes in producing, and of remedies in curing them, can be accurately stated. For instance, the epithelioma (or epithelial cancer, as it is often called) differs from cancer in its anatomical structure; for, instead of the cancer cell, the affected parts are infiltrated with abundance of scaly epithelium, larger than cancer cells, flat, folded, often adhering together, with less distinct nuclei, and very seldom a nucleolus. These also exude from a cut surface, mixed with a serous liquid, and constituting a *juice;* but this juice differs from the cancer juice in being not so equally diffusible through water, and in the fact that the cells adhere in clots or pellets. They are also often found rolled together in large concentric masses. This disease usually seems to begin in the rete mucosum, by a destruction somehow of that limitary membrane, which confines epithelium to the surface. As it differs from cancer in its anatomical elements, so it does likewise in its favourite seats, in its etiology and clinical history, and susceptibility of cure. It is apparently much more liable to be produced by local causes. It almost invariably commences on cutaneous or mucous surfaces, such as the lower lip, penis, scrotum, tongue, larynx, &c. It is excessively liable to relapse if cut out, but, so far as is known, always comes back in the part first attacked, or in the nearest lymphatics; and does not, like cancer, become diffused over the body. Lastly, there is reason for believing that, in epithelioma of *certain parts* of the body, excision may probably lead to a perfect and lasting cure. In the same manner, the fibro-plastic tumours may be defined as consisting of small round or oval cells, with small flattened nuclei, intermixed with intercellular fibrillary matter. The cells and nuclei may be in various degrees of elongation, and the fibrillary matter tough as hemp, or soft and flickering like jelly. Glandular tumours are composed of tissue, like that of secreting glands. Many other varieties of growth might be so defined; but my present purpose will be answered if I show that the plan of classifying according to structure is simple and intelligible, and permits the clinical and vital history of each growth to be fairly studied and compared.

On the other hand, the plan of classifying growths, not according to their structure, but according to their vital characteristics, is based upon the doctrine stated by the most illustrious of English pathologists, that we ought to 'choose modes of life rather than structure for determining the affinities of

morbid products, and for arranging them under generic names. As of all tumours, so especially of cancers, the true nature is to be apprehended only by studying them as living things.' Or, as the doctrine is still more broadly stated by some of my professional friends, if any tumour behaves as cancer does, it is so far 'malignant' or 'cancerous.' For instance, a glandular tumour is removed, and never returns; this is a glandular tumour *pure et simple*. Another glandular tumour returns after extirpation, again and again. This is said to be 'malignant,' or to 'partake of malignancy,' or to be 'disposed to take on malignant action.'

Now, after careful study of the subject, the continued use of the word malignant, instead of structural terms, to characterize morbid growths—and this in spite of the unanswerable objections of Walshe, Lebert, and others to the word—seemed to me at once so illogical and mischievous, that I determined to bring the subject for discussion before the Medical Society of London, in the hope of inducing my professional brethren to agree with me in the necessity of stricter terms. In the course of my investigations, I endeavoured to analyze the term malignant into the several qualities which, when combined, constitute 'malignancy;' and, at the same time, I endeavoured to frame a statistical table of morbid growths, which should show how far, and in what proportion of cases, the separate items of malignancy were exhibited by each growth. I soon found that accurate data for such a table are not yet to be had; but the analysis of malignancy may be of some use, as exhibiting certain pathological facts in a new light.

The vital characteristics of cancer may be described in about fourteen terms, the sum of which equals the word malignant. They are: 1. Constitutional Origin; 2. Rapid Progress; 3. Constant Progress; 4. Pain; 5. Return after Extirpation; 6. Secondary Deposits; 7. Diffusion; 8. Cachexia; 9. Resistance to Treatment; 10. An Infiltrating Mode of Growth; 11. Degeneration and Softening; 12. Ulceration; 13. Invasion of Lymphatics; 14. Heterology of Structure; 15. Death. As an accidental and occasional accompaniment, Inflammation or Hyperæmia deserves one word of notice.

1. *Constitutionality of Origin* is the first of these characters of malignancy; and since even now we sometimes hear the opinion that cancer is at first a local malady, that it contaminates the system at large only after a certain length of existence, and that, by means of early extirpation, this contamination may be prevented, we may state that the reason why cancer is called constitutional is, because, in an infinite majority of cases, if true cancer structure exists, though it be completely extirpated so soon as conceivable, there will yet be a return either in the part first affected or elsewhere.

But it is frequently held, even by most eminent pathologists, that, whilst cancer is constitutional, other growths, such as the fibrous, are local in their nature; and, since confusion in the use of terms is one of the greatest obstacles to the progress of any science, let me, without going into scholastic subtleties, endeavour to give a precise definition of these terms. A local malady is one the causes of whose origin and continuance are strictly confined to the part affected, or one which, if caused at first by constitutional causes, wholly or concurrently, is no longer dependent upon them. A constitutional malady, on the contrary, has its origin in causes affecting the whole body; or is perpetuated, extended, and maintained by such causes, even though its first origin were local. But, when we speak of causes affecting the whole body, we must refer either to deviations from the healthy composition of the blood, and of the other parts of the material fabric; or else to disturbances in the laws which govern growth and development; although we can hardly separate our ideas of such laws from our idea of the material substances by and upon which their existence is made known to us.

Now, tried by these definitions, I can scarcely conceive of a morbid growth which has not at least a concurrent origin in constitutional causes.

Take the case of the most innocent and local tumours, as they are reputed—those fibrous or fibro-plastic tumours which are caused by local irritation. Thousands of women have their ears bored for ear-rings; thousands of children have leeches applied; and men are cupped often: yet it happens as a most rare

case that a fibrous tumour is developed in the bored lobules of the ears. I have seen raised circumscribed fibrous lumps in the site of each leech-bite in a child, and similar ridges in the site of an old cupping; but, as these effects are not produced on all patients by these local injuries, there we must admit some constitutional peculiarity in the patient to account for them, or else we must abandon the doctrine which is the foundation of all physical science, that like causes produce, *cæteris paribus*, like effects.

I will pass over the fibro-plastic tumour, since it is generally admitted that many of the deposits which come under this head are connected with some cachexia, perhaps the syphilitic or scrofulous; merely adducing as an example the well-known *tarsal tumour* which infests the eyelids, and the origin of which I believe to be always referable to some degree of debility or cachexia. Certes, I have known patients affected by them at one period of their lives, and quite exempt at others. But I will adduce the glandular tumour, of which a fair example is to be found in the common bronchocele. Of this, a true tumour, consisting in exaggerated development of gland tissue, whether it be of the severe endemic sort prevalent in mountainous countries, or of the milder variety which may affect women anywhere, it is impossible to deny the constitutional origin. In every case which I have seen in London, there has been some concurrent deviation from health, and always on the side of deficiency—anæmia, atonic dyspepsia, or amenorrhœa. I have patients in whom the earliest sign of getting out of health is enlargement of the thyroid gland, and in whom that enlargement subsides as the health improves. In one lady, it is always caused by a residence at the sea, and disappears when she removes inland. The connection of the chronic mammary tumour (as the abnormal development of gland tissue in the breast is called) with deviations from health is sometimes palpable, often not so; but the true idea of the significance of such tumours is, as it appears to me, to be found in the study of the origin of the same tissue when occurring normally. Whatever meaning may be assigned to the term constitutional, there is no act in the whole career of life so truly constitutional as the development of gland tissue. From the Wolfian bodies, the thymus, and the supra-renal capsules, to the later developed sexual glands, there are no structures in the body whose development and decay are so inseparably connected with the most general laws of growth, and with the composition of the entire solids and fluids, as the glands; and what may be said of the normal, I do not hesitate to apply to the abnormal development of gland tissue.

Space compels me to pass over the bony, the cartilaginous, and other tumours, reputed local, with the observation that it is most difficult to conceive of hereditary, multiplied, and symmetrical tumours, as merely local: I must conclude this part of my subject by stating my belief that all tumours, properly so called, are constitutional. In what respects they differ, in this quality, I shall say presently.

2 and 3. *Rapidity and Constancy of Increase.* I may pass rapidly over the second and third signs of malignancy, because, though more characteristic of cancer than of any other growth, yet they have little absolute diagnostic value, and afford scope for little remark. Not so the fourth characteristic.

4. *Pain.* This is a symptom which attends morbid growths of all kinds, in differing degrees, and the *rationale* of which is not evident at first sight. Now, exclusive of pain depending on accidental inflammation, or on pressure or distension, or on the existence of an ulcerated surface, all of which kinds of pain may be common to other morbid growths as well as to cancer, there is another pain, of a purely neuralgic sort—stabbing, lancinating, or aching. But, although this is not so characteristic of cancer as it is of certain small fibrous tumours which, from that very circumstance, are called *painful subcutaneous;* and, although glandular tumours of the breast, and other tumours elsewhere, are sometimes accompanied with extreme neuralgic pain, yet it is extremely valuable as a diagnostic sign, and especially in the case of secondary cancer. This was pointed out to me long ago by my friend Dr. Ferguson, from whom I derived the following most useful practical rule: If, after extirpation of a doubtful tumour, the patient complains of neuralgia, secondary cancer may be

predicated. By this rule, Dr. Ferguson was able to give a diagnosis in a case of cancer within the skull, at a time when the doctrine of the painfulness of secondary cancer was not generally received by the profession. Of neuralgic pain, too, there are many varieties. Dr. Ferguson believes that, at the time of the formation of secondary deposits of cancer, much of the most acute and agonizing pain may be owing to the presence of cancerous plasma within the capillaries—a thing rendered probable by the intense pain caused by the injection of morbid fluids into the arteries of animals. At all events, the significance of pain, as a rational sign of cancer during the latter two-thirds of its existence, is scarcely to be overrated.

5. *Return after Extirpation.* In speaking of the fifth malignant character, a distinction must be drawn between tumours which return because some portion of the original is left to repullulate, and others which return in spite of the freest extirpation; but yet the doctrine so often insisted on by Lebert, that the return of non-cancerous tumours is generally due to incomplete extirpation, is, I think, scarcely consistent with true pathology. At the same time, this doctrine tends to keep out of sight the true facts relating to the return of tumours, which, I think, exhibit strongly their agreement in constitutional origin, and their differences; which may be summed up in a formula something like the following, viz: That, of some tumours, the constitutional causes are transitory; the morbid material is consumed, the necessary conditions are exhausted, the aberrant laws fulfilled, by the very production of the tumour; which, if then extirpated, is no more reproduced than an arm or leg would be; that, of other tumours, the constitutional cause, once established, is permanent; and extirpation is sure to be followed by reproduction.

That there is some common bond of connection between various tumours, especially the glandular, cartilaginous, and epithelial, is another proposition which may be proved at the same time.

On tabulating a certain number of cases of tumours of external parts (not including the uterus or viscera), including those mentioned by Paget in his Lectures, in the Catalogues of the Museums of St. Bartholomew's, and of the Royal College of Surgeons, many from Hughes Bennett, Lebert, and Warren, and from the medical periodicals and private sources, it appears that there is no tumour whatever which may not return after extirpation, even though the general characteristic may be the reverse. No better example can be desired than the case related by Paget,[1] of a fibrous tumour, yielding gelatin on boiling, removed twice, and returning, whilst similar tumours were developed in the lungs, as was ascertained by *post-mortem* inspection. In this series of cases can be found a dozen instances of such tumours removed twice, one instance of six removals, ten instances of cartilaginous tumours returning, and so forth. The true conclusion seems to be, that return after extirpation, from a continuance of the original cause, is a thing *possible* in the clinical history of any tumour whatever, and deserves to be known and commented on as a fact. But when we find the fact of recurrence made by some pathologists a ground of classification, and read of subspecies of 'recurring cartilaginous,' 'malignant glandular' tumours, &c., we must say that this fact is no better a ground for classification than if some botanist, disregarding structural affinities, should erect the *triticum repens* or horseradish into separate divisions from their kindred species, on the plea of the difficulty of uprooting them from the soil.

The comparison of the same series of cases tends to show that there is some alliance or connection between the causes of various kinds of tumours. This is evident from the instances of various growths combined in one tumour, of enchondroma combined with cancer (or, in the testicle, with any other growth), of glandular tumour, and of epithelioma combined with cancer. It is evident, too, from the instances of one sort of tumour succeeded after extirpation by another. For instance, in the St. Bartholomew's Catalogue; a glandular tu-

[1] Lectures, ii. 153; also Catalogue of the St. Bartholomew's Hospital Museum. For particulars of the cases referred to, see Paget, Lectures; Warren on Tumours; Hughes Bennett on Cancer and Cancroid; Hannover, Epithelioma; Catalogues of the Museums of St. Bartholomew's and of the Royal College of Surgeons.

mour of the breast, excised; tumour returns in opposite breast; excised; followed by a tumour in the former breast, half fibrous, half medullary; excised; followed by a brain-like medullary tumour on sternum. The affinity of various tumours, too, is evident from the instances of enchondroma and other tumours occurring in persons of cancerous families, and from the occurrence of cancer in persons whose parents had died of epithelioma. Lastly, it is evident from the acknowledged frequency with which cancer and epithelioma supervene on warts, moles, and nævi. Now, since this fact, and since the true nature of warts and moles are, from their very triviality, overlooked, let me observe that they are true fibrous or fibro-plastic tumours of the skin, which, in warts, are accompanied by hypertrophy of papillæ; in black moles, by deposit of pigment; whilst in the common raised smooth mole, there is a distinct deposit of a fibro-nucleated matter within the meshes of the cutis, but without the black pigment. The fact that these growths are not mere hypertrophy was pointed out many years since by Müller. Now, these growths are the favourite seats of epithelioma, of the corroding ulcer or lupus, and of cancer, especially of the soft and black varieties—a thing commonly attributed to 'malignant conversion.' The only true explanation I believe to be, that the constitutional causes of cancer or of epithelioma are superadded to those of the growth which existed before, which proves alliance between them, but neither identity nor conversion.

6. *Diffusion;* and 7. *Secondary Deposit.* By diffusion is understood a multiplicity of tumours arising from increase of the *materies morbi;* and by secondary deposit is understood the deposit into a fresh place of materials absorbed from the first seat of the disease. Paget noticed secondary deposit in two out of seven cases of epithelioma; and in the College of Surgeons' Museum, there is apparently a case of secondary deposit from epithelioma of the tongue of an ox. But, as a rule, cancer is the only disease which presents either diffusion or secondary deposit; in the case of the fibrous and other tumours, it is a rare exception; but it is from exceptional cases that the true limits of natural laws are to be defined.

8. *Cancerous Cachexia.* This characteristic is, I believe, much more indebted to imagination than to observation, for the colours in which it has been painted. As regards myself, I can gather no other conclusion than that the yellow anæmic hue, the emaciation, the sickness, loss of appetite, diarrhœa, despondency, and sleeplessness, are in most cases distinctly traceable to two sources: first, the pain, discharge, and irritation, which produce in the cancerous neither more nor less than the amount of cachexia which they cause in any other cases; and, secondly, the interference with the functions of internal organs. I believe that cancer may, and usually does commence, not only without cachexia, but with an apparently good state of health; therefore, that the cancerous material is not poisonous *per se;* that in most cases the cachexia, in other words, the decay of health, is directly proportionate to local mischief; that it is diminished by those measures which remove or palliate local mischief: yet that in some few cases, there is a degree of cachexy apparently disproportionate to the local disease. This, in my search after positive results, is the best conclusion I can come to. Certes, within this year I have seen a man dying of cancer of the stomach, whose life was slowly drained away by inanition; and, when it is considered that the only support for four months was given in the shape of enemata, it does not seem that the cachexy was of a very mortiferous sort. At an early period of my studies, I was struck with observing that cancerous patients were not by any means infallibly marked with visible cachexia; and this in a part of the country where a yellow waxy hue of the features, emaciation, bloodless lips, and other signs of impoverished blood, are so common in the peasant women after forty, that, as I pointed out to a very intelligent surgeon, one-half of the women past maturity whom we met might sit for portraits of the so-called 'characteristic aspect of malignant cachexia.'

9. *Resistance to Treatment.* Passing over this characteristic, that no medicine exists capable of causing a cancerous growth to wither, it may be remarked that the same is equally true of other growths, except those fibro-plastic growths connected with common inflammation, or with syphilis or scrofula,

some glandular tumours, and a few vascular. But it is a very curious circumstance, the morbid deposits which we have most power over, are precisely those which resemble cancer the most in their cell constitution, and in their infiltrating properties.

10. *Infiltrating Mode of Growth.* On this, as a practical diagnostic sign, too much stress can hardly be laid; as a philosophical ground of classification, none at all. I allude to the infiltrating properties of cancer as contra-distinguished for the circumscribed nature of other growths. So much importance has been attached to this by an eminent pathologist, that it enters into his very definition of cancer. But, on stricter inquiry, it will be found that infiltration, like many other properties, is accidental or occasional; that all cancers do not infiltrate, but may be circumscribed, even to a late period of their existence, which is particularly the case with soft cancer: and secondly, that other growths do infiltrate. The common fibro-plastic or inflammatory exudation infiltrates all tissues; so does tubercle notoriously. And lest it may be alleged that there is a difference between the mode of infiltration in these cases, I may state that I have made this the subject of special examination, and have found that whether muscle, for instance, be infiltrated with fibro-plastic or cancerous matter, the process is identical. The muscle, to the naked eye, looks pale and fatty; the fibrillæ are found to be losing their clear markings, and to be infiltrated with oil near the effusion, whilst in the very seat they are separated and broken up, in the one case by cancer, in the other by fibro-plastic cells and nuclei, and by much unformed matter; by the increase of which the fibrillæ at last disappear.[1] When, therefore, we hear of the infiltrating properties of cancer, of its being *substituted* for other tissues, or of other tissues being converted into it, we may ask how, in kind, this infiltration, substitution, and conversion, differs from that which goes on in the tissues around a spot of common inflammation; from the prolonged action of which, as we well know, all tissues whatever lose their identity, and become converted into a puriform fluid in acute, or gelatinous mass in chronic inflammation.

11. *Softening.* This process has heretofore even been enumerated amongst the defining marks of cancer, and almost as a speciality. It was formerly stated that the difference between cancer and other morbid growths consisted in its being unable to maintain its vitality beyond a certain period; after which it softened and underwent a process of ulceration; the older portions perishing as newer portions were being deposited.

But we find as a matter of fact, that the kind of softening which is most usual in cancer, is that which is met with in all other tissues natural and morbid, and which is commonly known by the term fatty degeneration. A cancer in this condition exhibits to the naked eye spots, streaks, and patches of softish yellow matter; on examining which the cancer cells are found either shrivelled, or filled with oil-globules, or broken down into an amorphous mass of granules.

But this softening from fatty degeneration is common to all other growths without exception. For instance, Mr. Haynes Walton lately sent me a tumour which he had removed from the nose of a young man, quite unadherent to the skin and periosteum, of five years' duration; it was composed of spherical masses of cartilage, some clean, firm, and pellucid; others almost liquid; others soft, yellow, and cheesy; the last were found to be mixed with granular masses, oil-globules, and cholesterin. There is no morbid deposit, or scarce any, in which similar changes may not be found; but there is one, in which it is so early and so constant, that it has been considered a speciality apart. I need hardly say that I refer to the so-called yellow, cheesy tubercle of the lung; which, heretofore regarded as a definite primary morbid product, must now be considered only as a degraded condition of some pre-existing material; and truly we want some new name for the product, of which the cheesy stuff is a degeneration, or else, if the original matter be still called tubercle, we want a name which shall express that cheesy degenerate condition which, as I insist, is no more peculiar to tubercle than it is to cancer.

[1] See a drawing at p. 24 of the Surgeon's Vade Mecum. Sixth edition.

But there is another kind of softening which may affect cancer, viz: the liquefaction of the intercellular structure, which converts what was a solid mass or sponge, with cells in its interstices, into a milky liquid with cells floating independently. Here springs up a whole crop of pathological analogies. The liquefaction of the fibrillary constituent of an ordinary sthenic inflammatory effusion constitutes common pus; that of other deposits constitutes *pseudo-pus;* and it may be said that there are as many kinds of pseudo-pus, or puriform fluid, as there are of effusions.

The nature and alliances of these kinds of softening will be illustrated by the case, of which the following is an abstract. For permission to observe the patient and quote the case, I am indebted to my friend Mr. Henry Lee, under whose care the patient was in the Lock Hospital.

Case.—M. C., a girl aged 21, was received into the Lock for great enlargement of the labia, with warty growths, consequences of repeated gonorrhœa and syphilis. Mr. Lee removed a large mass, by incision, from the right labium; from the wound he hooked out, with the handle of the scalpel, considerable quantities of soft and rotten tissue; on examination of the excised mass, it was found infiltrated with epithelium, and large portions of it completely softened down. The wound healed. Bright's disease of kidneys and dropsy supervened. Death occurred from pneumonia, and sloughing of the groins and labia. On *post-mortem* examination, several lymphatic glands in the posterior mediastinum were in a state of liquefaction, as well as the superficial glands in the right groin, and the sloughing tissues. When I say liquefied, I mean softened into a pseudo-puriform fluid, not in a state of fatty degeneration.[1] This case proves that large masses of tissue may be in a state of complete rottenness from degeneration, and yet cause neither abscess nor ulceration.

12. *Ulceration* is the twelfth alleged characteristic of cancer. But although it is perfectly true, as Lebert says, that hard cancer of the breast and stomach do usually ulcerate, yet this is not true of cancer in general; and when ulceration does occur, it is rather as a mechanical result of distension or of infiltration of the skin, than an essential property of the cancer itself. Tubercular deposits are more prone both to softening and to ulceration. But there are three affections often confounded with cancer, in which ulceration is the special mortiferous element. These are epithelioma; and those deposits in the skin, or mucous membrane, on which there has supervened that most intractable ulceration known as the *corroding ulcer* of Sir C. M. Clarke; the *lupus* of various authors; the *esthiomène* of Huguier. There is the superficial lupus which produces wide-spreading, shallow ulcers on the face, or on the female genitals. There is the exudent lupus or *esthiomène perforant*, which, in the face, usually attacks an old mole, or some new tubercular elevation, filled with cacoplastic material; and on the female genitals usually affects parts that have been infiltrated with similar deposit. In the face, it may destroy everything before it to the base of the skull; in the female genitals, it may perforate the vagina, bladder, rectum, and lay open the peritoneum, or even cause, by contiguous irritation, a fatal peritonitis, if life is not worn out first by pain and suffering; but yet this truly malignant affection has nothing of cancer but its local inveteracy; it does not affect the lymphatics, nor appear in any other part but that first attacked. Again, in cases of epithelial disease, corroding ulceration may supervene, and the lymphatics may be infiltrated; but the disease kills by its effects on the first-attacked spot, and does not kill by secondary deposit or diffusion, as cancer does.

13. *Infiltration of the Lymphatics.* This characteristic of malignancy is confined to cancer, epithelioma, tubercle, and cacoplastic ulcerations, including the syphilitic.

14. *Heterologousness, or Heteromorphosity of Structure.* Of this supposed characteristic of cancer, I shall only say that they are two admirable Greek works, and serve like many other things said respecting cancer, to keep up a

[1] I must take some other opportunity of publishing some remarks on pseudo-pus, puriform fluid, &c., which space does not allow me to introduce here.

sort of romantic feeling, injurious to true practical knowledge. That the cancer cell is not quite like anything else may be admitted; but so are some cartilage cells unlike anything normally found in the adult body.

As an accidental accompaniment of cancerous deposit, I am lastly bound to mention inflammation or hyperæmia. As in various processes plastic effusion may be or not accompanied by this condition, so may or may not cancer. The following case may be an illustration:—

Case. M. B., aged 40; soft cancer of skin of chest supervening on mole; enlarged axillary glands; excision; cure; diminution of glands; severe neuralgic pain; incipient repullulation of cancer *in situ;* relief to neuralgia by cauterizing the newly sprouting warts; fatigue just before menstrual period; sickness, abdominal tenderness and signs of hepatitis; jaundice; sudden enormous swelling of liver; inflammatory symptoms subside; slow death by inanition from cancer of liver.

Having thus gone through the alleged characteristics of malignancy, we find, on summing them up, that cancer is the only malady which possesses them all; that epithelioma, the Greek elephantiasis, lupus, and tuberculosis, come the nearest to it; that the glandular, fibrous, cartilaginous, and fibro-plastic usually display only four or five; yet that whilst there are exceptional cases, which prove the possibility of a cure of cancer, so there are conversely exceptional cases which prove that tumours, usually innocent, may destroy life, in the same way in which cancer does.

Therefore, I argue that the vital characteristics of these growths being variable, and the structure constant, the latter alone can be taken as a basis of classification; and that, since cancer is, as a rule, the only *malignant* disease, using that word in the sense of incurable, it were better, when cancer is spoken of, to call it cancer; because, if the term malignant be applied to any other disease, it occasions confusion and mischief; it may lead to the belief that a disease is cancer, when it is not; and that it is incurable, when perhaps persevering treatment might be successful.

A good instance in illustration falls under my notice even whilst I am revising this paper. Within these few days, I have seen, in consultation with my friend Mr. Hunt, a man, aged 60, with an immense ulcer, which has destroyed the right half of the lower lip, and great part of the right cheek, laying bare the lower jaw-bone. It began thirty years ago, as a little indentation, covered with a thin scab, which became a foul ulcer, but, after a long time, healed. Again it broke out, and healed. On breaking out again, it was cut out by Mr. Bransby Cooper, and was well for a time. It reappeared, however; and the patient, amongst a multiplicity of medical advisers, before he came under Mr. Hunt's care, met with one physician who told him that it did not signify; and with a surgeon, who said it was 'malignant,' and ought not to be touched. There is, however, no sign of cancer about it; the ulcerated lip, at its edge, is as supple and thin as possible, and the only induration is in the periosteum at the edge of the denuded bone. I cannot help thinking that the case might have turned out more fortunately had the case been more perseveringly followed up. But, in truth, the word malignant, applied to a disease, is like the *Græcum est, non potest legi*, of the mediæval ecclesiastics; it is an excuse for non-interference; it appeals to the imagination, invests disease with mystical supernatural qualities. Thus the practitioner, instead of treating his patient, lulls himself to sleep with dreams of heteromorphosity.

But since, under the term malignant, have been grouped diseases palpably different, which nevertheless have all been condemned as hopelessly incurable (thus, even in a work published in 1853, cancer, epithelioma, and corroding ulcer of the female genitals, are all styled malignant, *or* cancerous *and* incurable), one practical benefit resulting from the disuse of this term will be greater care in discriminating the various species of disease, and in assigning to each its most appropriate treatment.

Thus, of the corroding lupus, it seems now a well-established fact, that efficient excision or cauterization may be followed by absolute cure. In the case of the late Pope Gregory XVI., an ulcer of this kind was eradicated from the face by the arsenical paste, and there was no return during the eight remaining

years of his life. In the corroding ulcer of the female genitals, too, thorough excision or cauterization have effected permanent cures.

In the case of the epithelioma, and of warty growths resembling it, which are liable to become the seats of this affection, there can be no doubt of the propriety of excision as early and as freely as possible.

In the case of cancer, the general practice of the profession seems, so far as regards extirpation, as sound in principle as the present state of knowledge permits it to be. Regarding the treatment of cases not capable of extirpation, I have reason to know that the vague term malignant, and the idea of death through cachexia, operate most unfavourably by inducing the surgeon to neglect palliation or treatment; but if the opinion were acted on, which I believe to be the just one, that cancer, as a rule, destroys life as other diseases do, by its local effects, then the surgeon would feel that every particle of skin he can save from excoriation, every portion of the foul and irritable ulcer that he can cleanse or soothe, will be so much taken from one palpable cause of death. I know of one case in particular, in which the more than commonly assiduous use of common means, especially blackwash and bismuth, was of immense service in prolonging life.

As to general treatment, much may be hoped from the persevering use of means to neutralize pain and exhaustion. The neuralgic pains, in the case last related, were immensely soothed by ether and warm negus, more than by opium, though this last, with chloroform, is unhappily the sheet-anchor.

At present, all treatment of cancer is palliative, and directed against local manifestations and their results. But, when we consider that, ten years ago, before Hughes Bennett taught us the use of cod-liver oil, consumption was almost regarded in the same light, we surely need not yet despair of finding some substance which may destroy and eliminate what is noxious, or supply what is deficient, and so bring this intractable disease within the proper sphere of therapeutics. That there is a potentiality of cure, is shown by the degeneration of actual cancers; that real cures may have happened, cannot be denied; but, before any fact can be held as certain, or can be estimated statistically, a reform of nomenclature and classification, and the disuse of that most noxious word 'malignant,' are essential."

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26. *Arsenical Injections in Cancer of the Uterus.*—Mr. Lloyd has for a long time employed, in cases of cancer of the vagina or uterus, which have come under his care in St. Batholomew's, injections of arsenical solutions for the purpose of preventing fetor. He states that such treatment is perfectly innocuous, and that it succeeds far better than the ordinary disinfectants, which at the best are but of very temporary benefit, the smell generally returning as bad as ever quickly after their employment. The strength of the solution used is from two to eight grains of white arsenic to the pint of water. The great difference between the chemical disinfectants, and such applications as this, no doubt arises from the circumstance that the one only destroys fetor, while the other prevents it. The extraordinary influence of arsenical applications in cleaning cancerous ulcers is well known to those who have tried them. It is not necessary that they should be made to act as escharotics, since repeated dressings with a paste only strong enough to stimulate will effectually serve the end. The following prescription is that of the powder used by Mr. Startin at the hospital for skin diseases for the purpose mentioned, and it differs, we believe, not very materially from one which was a great favourite with Dupuytren: ℞. Hydrarg. chloridi ℥iiss.; hydrarg. bisulphureti ℈ij.; acidi arseniosi ℥j. Misce.

This powder, made into a paste with water, is more or less lightly, as required, to be brushed over the sore, the application being repeated once a week, or fortnight, or month, as may be necessary. Mr. Lloyd's injection, in all probability, acts in the same manner on the sloughy, cancerous ulcerations concealed within the vagina. It must be borne in mind, some real good is probably produced in addition to the prevention of fetor, since the diminution of discharge and sloughing will save the patient's powers, and perhaps, also, somewhat retard the growth of the cancer.—*Med. Times and Gaz.* July 1, 1854.

27. *Amputation at the Knee-Joint.*—William Fergusson, Esq., in a clinical lecture lately delivered at King's College Hospital, related the following case of violent inflammation of the leg followed by necrosis, in which he amputated at the knee-joint, and remarks on that operation:—

"W. M., aged 11, is a native of Sydenham, and states that he has always had remarkably good health up to the time of his present illness, which began six weeks ago. After having been out sliding the whole of one day, he came home in the evening complaining of pain in both legs, more especially in the right knee, upon which he had fallen in the course of the day. In a few days after this he was seized with shivering and violent deep-seated pain in the right leg and ankle-joint, which was followed by considerable swelling of the limb, commencing at the ankle and extending up to the knee-joint. The integuments appeared red, as if very erysipelatous. His sufferings now became excruciating, more particularly if pressure were made on the limb, or if he attempted to move it. Notwithstanding the active measures employed by his own surgeon, the inflammation continued to increase, and matter formed, which soon became discernible in the soft parts. An incision was consequently made on the outer part of the ankle-joint, and about a pint of pus evacuated. A few days after this, another puncture was made in the upper part of the leg, and more matter was discharged. During this time, his general health had become much impaired, and he became extremely emaciated.

"When admitted into the hospital, January 25, 1854, Mr. Fergusson made an accurate examination while the boy was under the influence of chloroform, and found the knee-joint much diseased, the surfaces of the bones being rough and denuded, and a considerable collection of matter in the upper part of the leg, which was evacuated.

"The patient was supported by stimulants for a few days, till his health was deemed sufficiently good to stand the shock of an operation.

"When placed on the operation-table under chloroform, a small opening was made a little above the knee, and a quantity of unhealthy pus was evacuated. Mr. Fergusson then performed the operation of amputation at the knee-joint, in the way detailed in his own work.

"The state of the bones of the leg clearly demonstrated the necessity of their removal. A section being made of the tibia, the cancellous tissue of the upper part of that bone was found filled with pus, while that tissue at the lower part was necrosed, and the epiphysis separated. The articular cartilages of the ankle-joint had ulcerated, and the ends of the bones were eroded. The articular cartilage on the head of the tibia was so soft that a probe passed readily through it, and the bone was bare and carious in several spots, especially around the articulation with the fibula.

"The patient rapidly improved, and was discharged cured, March 11, 1854."

Now, if I be not mistaken, the late Mr. Liston performed a similar operation at University College Hospital; but I am uncertain as to its performance by that gentleman, or to the date, if it were performed. When I first performed the operation, to my knowledge it had not been previously done in England, for I cannot find any record of the fact, and such an operation performed for the first time would not likely be passed over without some notice being publicly made.

This operation has since been followed by others, among the first of whom I may name Dr. George Williamson, now in India, one of my former assistants. The first time I performed this operation was on the person of a full-grown man (the case is mentioned in the third edition of my work on *Practical Surgery*); and I have rarely seen or made a better stump. He has repeatedly walked forty miles a day, and once walked one hundred and twenty miles in three days; and, what is more astonishing, his false leg was but indifferently made and padded, the spoke of an old wheel being considered by the man an excellent substitute for a more expensive contrivance.

In consequence of such great advantages arising from my first trial of the operation, I have since frequently performed it. Notwithstanding, several objections have been made. Mr. Syme, who had performed the operation in Scotland before I attempted it here, had taken a dislike to the proceeding from something that went wrong in his own cases. Mr. Syme imagines that greater danger

is incurred by a larger surface of bone being exposed, by the removal only of the condyles, than if the bone be sawn higher up in the shaft. But mischief, I think, is more apt to occur when the bone is sawn in the shaft. Where the bone is vascular, I think there is little chance of necrosis, and much less of caries; and you have frequently seen how kindly the two cut surfaces of the spongy portions of bone heal in cases of excision of the elbow joint.

Another objection made is that the length of the stump is very awkward. This I do not admit; if the stump be short, an apparatus cannot conveniently be fitted, and the bone, when cut too high, is liable to be tilted forward by the psoas and iliacus muscles. Indeed, I cannot perceive any objection to a long stump. Objections have been made to a long stump of the leg, and amputation of the leg is often recommended to be done a short distance below the knee; but I am doubtful of the utility of such a step as a general rule.

Again, a long stump in the thigh can never hinder in any way; besides, the leverage is much greater than it if were only half the length. In addition, the great breadth of bone, when well covered, is better able to support the weight required to be borne.

This objection might be raised by some, that this operation is not truly an amputation at the joint, as the condyles were taken away. If such be allowed, then one might say that Mr. Syme's operation at the ankle is not an amputation at the joint, for he always removes the malleoli; but such an objection could not be held reasonable, and the operation now under consideration I deem a great addition to the history of amputation, and have taught it as such for the last ten years.

I now perceive that, for nearly the first time, mention is made of this operation in the surgical Journals of the day, which, I make no doubt, will greatly tend to the advantage of this department of surgery.

When I first commenced my profession, it was an understood rule, with but few exceptions, that the coverings of the bone in an amputation should be taken from the sound parts of the region when the operation was required; as, for instance, in amputation of the thigh, the soft parts are always taken from the substance of the thigh; so also in amputations of the leg. But in this case, and in amputation at the knee, the soft parts covering the end of the femur are actually the tissues that originally constituted the calf of the leg.

In the history of amputation, it has always been the aim of the surgeon to make a good stump, its quality depending greatly upon the proper covering of the bone. If the soft parts be scanty, a bad stump must result; if, on the other hand, the covering be too large, the result will likewise be unsatisfactory. A remarkable instance of this latter kind was under notice last summer in this hospital. But there is more danger of the covering being scanty than profuse. The fleshy condition of the covering, as you know, is ultimately converted more or less into a fibrous texture.

Though amputation cannot be said to be the opprobrium of surgery, an axiom I laid down in my first paper on Conservative Surgery—"For the greater proportion of sound material that we can save in any operation on the body, the nearer we come to the perfection of good surgery"—yet I think amputation at the knee-joint may fairly have at least a footing in the province of conservative surgery.

This operation has lain for some time in abeyance; but I now find my name associated with it in the journals, in papers which have recently appeared on the subject, from the abler pens of my friends, Mr. Greenhow, of Newcastle, and Mr. Jones, of Jersey, men who, with myself, I would fain hope, have no desire to have their names connected with novelties, unless it be for the good of our fellow-creatures and the advancement of surgery.

In cases of injury of the joint, including great contusion of soft parts, I am doubtful whether the operation should not be effected above the seat of injury. As to the mode of performing the operation, I first make a small anterior flap, drawing the knife across the front of the joint, and then, inserting the point of the blade behind the femur, thrust it through to the other side, close to the condyles; then, carrying it downwards, cut the posterior flap from the

calf of the leg. The saw is then applied a little above the condyles, and the flaps brought together as in an ordinary amputation.

My friend Mr. Greenhow, of Newcastle, saws through the bone before making the posterior flap; but I prefer the method I have described, although the great aim is to obtain sufficient material to cover the bone.

In some instances, I first effect the separation of the leg at the articular ends, and thereafter cut away as much of the femur as seems needful. In all cases, it is requisite to take the full length of the calf for the posterior flap, as the soft parts in the back of the thigh contract very much in the course of time. The patella might be saved in some examples, but in general I think it would be best to remove it.

I have been thus particular in my remarks, as you will not find any particular mention of the operation in any English work prior to my own.—*Med. Times and Gaz.* July 8, 1854.

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28. *Advantages of operating in certain cases of Harelip at a very early period.*—Henry Smith, Esq. advocates (*Med. Times and Gaz.* March 25, 1854) operating at the earliest period in certain cases of harelip. "It is obvious," he says, "in the first place, that an infant with harelip cannot so readily take in that nourishment which is offered by nature. If, however, the deformity be remedied, the child will be placed by the aid of surgery in a much more favourable condition to receive the nutriment afforded by the mother's breast. It is plain, too, that the deformity excites most unpleasing and painful sensations in the mind of the mother and those around her; and, if the source of this anxiety can be removed at once, it is of great importance that it should be accomplished.

A third argument in favour of very early operation for harelip consists in the circumstance, that as the growth of the child is very rapid in the first period of life, the lip, with other structures of the body at this time, becomes more fully and fully developed, and thus, after an operation has been successfully performed, there will be a much less chance of subsequent deformity in the part.

But it is in those instances where the harelip is complicated with a more or less extensive fissure in the palate that an early operation for the cure of the former is so imperatively demanded, and is attended with such beneficial results; and it is to this point especially I wish to draw attention, because, although in some recent works of surgery an operation at an early period after birth is recommended (and I may especially allude to the *Practical Surgery* of Professor Fergusson, and to the *Surgeon's Vade Mecum*, by Dr. Druitt), the most important reason for such a proceeding is not alluded to. And I now refer to the effect which is produced upon the fissure in the hard palate by the approximation of the edges of the lip.

As long as the harelip remains in its primitive state, there can be no pressure upon the hard tissues underneath; but, if it be united by the surgeon, a considerable amount of pressure is exerted upon the edges of the cleft in the palate; and, in a child aged only a few days or weeks, the bones are so soft and compressible, that they are to a great extent influenced by the pressure which constantly obtains, and in the course of time the fissure becomes either entirely closed or diminished in size to one-third or one-fourth of its original extent.

I have had various opportunities of noticing this effect in instances where a very early operation has been performed for harelip, complicated with more or less extensive fissure in the hard palate; and so convinced am I of the importance of performing the operation as soon after birth as possible, that I invariably recommend it. And it has fallen to my lot to be called upon to perform the operation very soon after birth, where there has been, at the same time with the harelip, considerable malformation of the palate; and I have been able to notice the results some length of time afterwards.

More than three years ago I operated upon an infant only four days old; here there was an extensive fissure extending through the hard palate into the nos-

tril. I had an opportunity of seeing this child only a few days since, and the opening in the front portion of the palate was closed up. In this case the soft palate was extensively cleft, and that still remains open; but the parts altogether are in such a condition that, some years hence, they may be completely closed by staphyloraphy.

A few weeks since, a little patient was brought to me, on whom I operated at a very early age, two years ago. In this instance there was a fissure in the hard palate, and great deformity of the jaw, a portion of which I removed at the time. There is now an admirably developed upper lip, and complete closure of the opening which existed in the palate.

In another instance, where I operated at an early period, there was an immense chasm running through both soft and hard palate into the nostril. I had an opportunity of seeing this patient a few days since, and found that the anterior portion of the cleft was much diminished in size. The operation was done more than a year ago.

Mr. Bateman, of Islington, who pays great attention to this matter, operated, three years since, upon an infant only four hours after birth. In this case there was an extensive fissure in the palate. This gentleman kindly showed me this case, and, in reply to my inquiry regarding the effect which the operation had had upon the palate, he wrote word the other day that the child had died of hooping-cough last winter, but that its mother remarked that before death the fissure, which had at birth been 'so large that she could put her thumb into it, had contracted so much that it would scarcely admit the edge of a sheet of writing-paper.'

About a month since I operated upon an infant only six days old, with perfect success. In this case I was partly induced to perform the operation at this early period because there was a fissure in the hard palate, extending into the nostril. I have little doubt that, in time, if the child lives, the fissure will be completely closed."

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29. *Paracentesis Thoracis.*—Dr. J. R. Bennett read a paper on this subject before the Medical Society of London (May 27, 1854), the chief object of which was to inculcate the importance, in cases of inflammatory hydrothorax, of not hastily resorting to the operation of paracentesis. He founded his objection to this proceeding in the early stage of the disease, both on the non-necessity of the measure, on the amenability of the disease to general treatment, and on the mischief which was likely to arise from puncturing the cavity of the chest. He showed the non-necessity of the operation, by the relation of cases in which there had been a large collection of serum in the chest, but which had been absorbed by general treatment and the use of counter-irritants, consisting either of blisters, or of the application of a very strong tincture of iodine. The medicines administered consisted of very small doses of blue pill, with squill and Dover's powders, an infusion of cascarilla, with iodide of potassium, and sweet spirits of nitre. The patients were placed under non-stimulant but nutritious diet. He objected to the use of mercury—to the production of the specific effect of that medicine, which he regarded as injurious. He related a case to which he had been called in the country, of hydrothorax, in a young gentleman, in whom the symptoms were not of such an urgent character as to require operation. He recommended the employment of remedies similar to those which have been mentioned, and with every prospect of their being useful. Another physician was called in, however, before a fair trial was given to the measures proposed, and paracentesis was performed. The fluid was serum; but on a second operation being required, about three weeks afterwards, the matter evacuated was purulent. This illustrated one of the dangers to which tapping exposed a patient suffering from hydrothorax. With respect to the diagnosis of the nature of the fluid in the chest, this could be determined without danger by the passage of an exploratory needle. If the fluid were found to be purulent or contained albuminous flakes, he recommended a gradual and continued drain of the fluid rather than its sudden removal.—*Lancet*, June 10, 1854.

30. *Excision of Ovarian Tumours.*—Prof. JOHN ERICHSEN, in a paper on this subject read before the North London Medical Society, makes the following remarks:—

"In considering the subject of ovariotomy, two questions present themselves: 1. As to whether the operation is a sound one and ought to be retained in practice; and 2. If retained, in what class of cases it should be had recourse to?

"In answering the first question, we must refer to the course that is taken by ovarian tumours, and to the result of the operation as deduced from published cases.

"The course taken by ovarian tumours necessarily varies greatly. In some instances, these growths do not appear to be incompatible with prolonged existence and a fair share of health. In other instances, again, after remaining quiescent for some length of time, they assume considerable activity of development, and interfere so seriously with the other abdominal organs and with the general operations of the economy, that life is attended by great misery and discomfort; and in a third class of cases they rapidly and steadily run their course to an unfavourable termination.

"After a time, in the majority of instances, the tumour may be materially lessened in bulk by tapping; but when once this operation has been had recourse to, it will require to be repeated with increased frequency, the intervals between each succeeding tapping being diminished; and then a fatal termination at a comparatively early date may usually be looked for. Mr. S. Lee states that of forty-six patients who were tapped, thirty-seven died, and only nine recovered; and that of the thirty-seven who died, more than one-half did so in four months from the first tapping, twenty-seven out of the thirty-seven in the first twelve months; and of these, eighteen were only tapped once.

"The objection has been raised against ovariotomy, that the mortality from it was so high as not to justify a surgeon in performing it. Undoubtedly, a very high rate of mortality after an operation would constitute a serious bar to its performance; and the more so, if it could be shown that the disease for which it was practised was not a necessarily fatal or even a very serious one. Thus, Mr. Phillips has collected the particulars of 61 cases in which ovarian tumours were extracted: of these, 35 were successful, and 26 died. Mr. Lee gives 90 cases; of these, 57 recovered, and 33 died: and Dr. Robert Lee, the most recent writer on the subject, has collected 102 cases of ovarian extraction, of which 60 did well, and 42 terminated fatally. From these statistics, it would appear that the mortality after the removal of ovarian tumours amounted to rather more than one in every three cases. This rate appears to me not to be excessive, when we compare it with the result of operations for various severe surgical injuries or diseases.

"The next question that occurs in connection with ovariotomy is as to the class of cases in which it should be practised. Now here we come upon ground in which much difference of opinion must necessarily exist. Ought ovariotomy to be performed in all cases, or even in the majority of instances of ovarian disease; or ought it to be limited to a few and exceptional cases, and practised as a last resource?

"The most ardent advocate for this operation will scarcely, I think, advise that an attempt should be made to extract the ovarium from every woman labouring under tumour of this structure; but would rather recommend the employment of palliative treatment, until the growth had begun to interfere seriously with the comfort of existence, or with the healthy action of the abdominal organs. When these injurious effects of pressure have once fairly begun to manifest themselves, the patient wasting, suffering much discomfort from her size, with difficulty in breathing, repeated vomiting, gastric irritation, &c., then the question of relief by operation will necessarily obtrude itself, and must be discussed dispassionately and without prejudice. Under these circumstances, I confess I see no chance of giving the patient relief, or of prolonging her existence, except by the removal of the tumour. Medical treatment is of no avail in such cases, and tapping only gives temporary relief, and will eventually, and often very speedily, wear out or destroy the patient. Under such circum-

stances, therefore, we must either leave the patient to her fate, or have recourse to the ablation of the tumour, which, it has already been shown, may be done with the prospect of success in nearly two cases out of every three.

"So far then as the strictly *surgical* aspect of this question is concerned, I should say that as the recoveries after this operation amount to at least 60 per cent., it is proper for the surgeon to propose and to perform it when all other means of relief have failed, and when the patient's health is giving way under the extension of the disease.

"There is one aspect under which we have not as yet considered this matter, and which has a most important bearing on the operation. I mean in its *diagnostic* point of view. This, which may with justice be termed the *medical* part of the question, and may be considered as belonging rather to the province of the physician than to that of the surgeon, has to be considered in two of its bearings: 1. As to the existence of such adhesions between the enlarged ovarium and the other abdominal organs as to render the extraction impossible; and 2. The diagnosis between ovarian and other abdominal tumours.

"The existence of such adhesions between the ovarian tumour and the abdominal viscera, as will prevent the possibility of its extraction, is of frequent occurrence, having been met with in rather more than one-third of the whole number of cases operated on; according to Dr. Lee, in 60 out of 162 cases. The frequency of the occurrence of this complication renders its diagnosis one of extreme importance. Its existence may in general be *suspected*, when it is ascertained that the patient has been the subject of attacks of peritonitis, when the abdominal tumour does not appear to change its position on the patient taking a deep inspiration, and then expiring freely; and when, on the patient's raising herself into the sitting posture, the sac does not tend to move forwards into the space between the recti muscles. So also much light may be thrown upon this important point by ascertaining the existence of a crepitant or crackling sensation between the anterior abdominal wall and the tumour; and more particularly, if when the tumour is tapped, it draws down over it the abdominal parietes, or sinks into the pelvis without exercising any traction on those parts. The condition also of the pelvic viscera, viz. the bladder and uterus, as ascertained by examination with the sound, may tend to show whether connections exist in this quarter or not.

"By such examinations as these, the presence of adhesions may doubtless in many cases be suspected; but in other instances, and more particularly when the mass is bound down posteriorly, there is no possibility of determining this point, and then the surgeon may, to his great annoyance, find that after laying open the abdomen, the operation cannot be completed, and the tumour requires to be left. In those cases in which there is any reason to suspect the presence of adhesions, it is, I think, a wise precaution to make a small exploratory incision into the abdomen, through which the fingers or hand may be introduced, and the connections of the tumour examined. If these are too extensive to admit of removal, the aperture may be closed, and possibly the patient may escape without any very serious consequences ensuing; as in these cases the peritoneum has in a great measure lost its character as a serous membrane, and is not so susceptible of the diffuse and destructive forms of inflammation that would otherwise be likely to occur in it.

"The diagnosis of ovarian from other abdominal tumours, falls so entirely in the province of the physician, that I do not feel myself prepared to enter upon it; but would prefer leaving that point to be elucidated by those members of the Society who have paid special attention to this subject."—*Assoc. Med. Journ.* Jan. 13, 1854.

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31. *Ununited Fractures.*—A lecture has recently been delivered at St. Bartholomew's Hospital, by EDWARD STANLEY, Esq., on the very practical subject of ununited fractures, and the various modes recommended and adopted to remedy this troublesome surgical disease. In an experience of sixteen years in the largest hospital in London, Mr. Stanley first remarked, either among his own cases of fracture, or in hundreds of other fractures under Mr. Lawrence, he did not recollect a single case of ordinary fracture treated in hospital which did not

turn out favourably. The cases of "ununited fractures," which he wished to speak about, had come into hospital *as such*, and in too many instances seemed all but incurable. Mr. Stanley seemed of opinion, from talking the matter over with other eminent surgeons—Brodie, Fergusson, Skey, &c., that a great deal could be done in such cases, but we should first understand the nature of the accident we have to deal with; what was useful in one case of false joint, or badly-united fracture, proving very little use very often in others. The first element in the calculation of all such accidents Mr. Stanley alluded to is that of *time*. In an ununited fracture of the femur, for instance, of a few weeks or months, we should still use steady compression on the parts by a leather splint, combined with the most absolute rest. We may find, in fact, several conditions of the party—for instance, an entire failure of the reparative process on the part of Nature, or what Mr. Stanley thinks, the real state of things: a piece of muscle gets torn and thrust between the ends of the bones, and prevents the callus thrown out provisionally from acting as it should in joining the parts thus separated. This, he is inclined to think, occurs oftener than is suspected.

2. We may again find, at a longer interval of time, after years of annoyance to the patient, the ends of the bones "eburnated" or polished, hard in texture—a rude capsule formed round a kind of joint, even new cartilage and a synovial-like fluid (Mr. Stanley exhibited several preparations of this kind from the museum of Bartholomew's—one very perfect; the humerus of a woman broken from slight muscular contraction of the deltoid, a subsidiary olecranch and elbow formed with capsular investments, formed at the insertion of the deltoid)!

3. Here is a class of cases (and they are those commonly found in hospital) where the process of union continues to all but the last or the stage of ossific deposit: you have a fibro-cartilage or cartilaginous formation, but no earthy matter. When we grasp the limb, and expect to find it firm, we find it to yield like a hinge, and a point on which Mr. Stanley lays great stress as diagnostic of false joint—there is always great PAIN. This is, in fact, the best test that union has not been completed. An elderly woman, now in Bartholomew's, shows this very well, as the least handling of the limb is agony to her. A popular idea would lead us to the belief that ununited fracture arises from debility or a cachectic habit of body; but, "in my practice," said Mr. Stanley, "I cannot say I have observed this; a man lately in hospital is the picture of rude health; and in fifty-six cases collected by Mr. Anneslie, fifty-two were strong, stout subjects, many occurring just at the middle period of life, when persons are most mixed up with the bustle of active life, and most subject to fractures—of course, exceptions may occur."

From the result of cases of his own, as well as of those under the hands of other surgeons of much experience, Mr. Stanley believes some "*local*" cause has more to do with the disease than anything of a constitutional or general kind; if the first period of irritation and of provisional callus forming after fracture passes by without a perfect joining of the two ends of a bone, the latter are likely to remain ununited until a new irritation is again set up in the parts. In a case now in Bartholomew's, which Mr. Stanley has been obliged to amputate, this local cause has been a large piece of muscle (the crureus) jammed in between the broken ends of the bone. The treatment of ununited fracture is one on which both Mr. Lawrence and Mr. Stanley can speak with no little authority, as in private practice as well as in hospital, they have seen everything tried, and almost everything fail or succeed. Mr. Stanley mentioned more than twenty methods of treating ununited fracture, but referred them all to the following heads: Firstly. As most useful, perhaps *compression* and absolute *rest*, effected by different arrangements of stiff leather splints, felt, gutta percha, &c. After sixteen years experience of this plan at Bartholomew's, he thinks such means scarcely ever fail. The chief difficulty is to procure perfect immobility of the ends of the bones, prevented by the movement of the joints next the fracture. This must be particularly remembered, or all our ingenuity is useless. Secondly. *Rubbing the ends* of the bones together. This will be found a most useful adjunct, but is chiefly to be relied on in recent cases before you have a false joint and synovial cavity with cartilage. It may be done now more carefully, too, with the aid of chloroform, and should always perhaps pre-

cede more active modes of treatment. Thirdly. Cutting down and applying *caustic*. This plan has been tried, but Mr. Stanley does not at all recommend it. You have exfoliation, which may be troublesome, with very severe inflammation, &c. Fourthly. *Sawing the ends* of the bone. This is generally called for only in very old cases, where there is a false capsule, and a rude kind of false joint to contend against. This, too, can be now well performed under chloroform. Fifthly. *Setons*. Some cases treated in this way, under Mr. Paget, have done very well. A sixth mode, that by *ivory pegs*, he has lately tried; but it must be confessed with indifferent success. The author of this plan was Dieffenbach, of Berlin; he noticed in some gunshot wounds, that where bullets and other foreign bodies had lodged in bones, a large quantity of healthy osseous matter was thrown out, as if nature tried to bury them, and prevent irritation of parts in juxtaposition. Mr. Stanley exhibited a curious specimen of this kind, from his private collection. A soldier, wounded at Waterloo, received a ball which went in at the back and stuck in the posterior part of the sternum. The man lived a long time, for the bullet is half buried in *new* osseous structure, thrown out around it. Dieffenbach at once tried the plan; and with a common gimlet, a few pegs, and proper splints and bandages, succeeded quite wonderfully. Two other modes, drilling holes through the ends of the bones, and then keeping them in contact by *wires*, and also another, that by *blisters*, Mr. Stanley seemed to think had not much in their favour, the wires keeping up too much irritation, and the blisters acting perhaps, like ordinary setons—the latter a plan very deserving of trial before recourse is had to other means.

Mr. Stanley concludes his lecture, by citing two cases in his practice by the ivory pegs: one very successful; the other now in St. Bartholomew's, not so encouraging, but where it would appear that the process of reparation was prevented by a cushion of the crureus muscle getting between the ends of the bone, as shown by the leg after amputation, performed last month (June, 1854), a large quantity of bony matter thrown out round the pegs.

Case I. Among his notes, Mr. Stanley finds the following case: About four years ago a man was brought into St. Bartholomew's (Bentley's ward). He had been working on the railway, and sustained a very severe injury, fracture of a compound character of both tibia and fibula, if we say he was lying on his back eight months; and at the end of this period, though the external wound was healed up, yet the union of the bones was not complete; it is quite sufficient to indicate his condition and the very tedious nature of the case. In a word, it seemed one of those cases which to Pathological Society men, would seem doomed never to get well. The man at length got about on crutches, but it was with most dreadful pain and torture of the limb, so that he at length gave up, wishing his leg to be cut off. It was decided, however, in consultation, to do something for the cure of ununited fracture. The first plan tried was a succession of blisters for a month, with no equivalent benefit; the limb was then encased in strong leather, stiff, and about as immovable as the limb of an Egyptian mummy: but this plan did not prove either very successful. After ten or twelve weeks, the question came up really of amputation; but this time with the rather fanciful alternative of ivory pegs, as suggested by Dieffenbach. The ivory pegs succeeded perfectly; and Mr. Stanley has the pleasure of seeing the man constantly brought to new life and activity, by four of these rather incongruous remedies borrowed from a cribbage-board! Four were inserted, one in each bone, at each end of the fracture: small holes drilled with a common drill, respectively, in tibia and fibula, then the pegs tapped well in with something like an auctioneer's hammer; and here Mr. Stanley said, perhaps, the most curious part of the matter is worth observing, in a physiological point of view, observed also in a similar case at Leeds; one peg was entirely absorbed, the other appeared eaten away, like carious pieces of human bone! When withdrawn, the small head of the ivory peg left out of the bone through the integuments was not changed at all. The successful issue of the case (after a whole year's unsatisfying ordeal in hospital) was most grateful, and the man left town quite well. In the second case, marked by all Mr. Stanley's kind attention and care, the result has not been so satisfactory; it has been a case brought into hospital, of long standing, of ununited fracture, high up in the shaft of the femur, both

ends freely movable and grating together. He first tried pad and splints, next inclosed the limb in stiff leather case twenty months. After injury, the question of amputation, or ivory pegs, as in the former case, was again considered: the pegs were tried, but the quantity of bone thrown out was of no use, and amputation, as a last resource, was performed.—*Dublin Med. Press*, August 2, 1854.

OPHTHALMOLOGY.

32. *Facial Anæsthesia, with Simultaneous Destruction of the Eye.*—The following extremely interesting case of this character is recorded (*Med. Times and Gaz.* 15th July, 1854) by Dr. R. Taylor, Surgeon to the Central London Ophthalmic Hospital.

Eliza Martin, aged 46, became an out-patient of the Central London Ophthalmic Hospital, March 17, 1853.

Six months previously, after exposure to cold and wet, she had been confined to bed for a fortnight with a violent cold, and pains all over the body. On awaking one morning, she felt her left eye painful; on putting her hand to the part, she found that the feeling of the whole of that side of the face was gone, and on examining further, she discovered that the features were drawn to the same side, the eye blood-shot, and the vision impaired. The pain soon became very severe, radiating all over the side of the head and face, and in a few days the sight of the eye was completely gone. She was then received into St. Bartholomew's Hospital, where she overheard her case, so far as the eye was concerned, described as one of abscess of the eyeball. She remained in the hospital for six weeks, during which the pain completely left her, and the eye, which had been very prominent, returned nearly to its proper position in the orbit; the sight, however, did not return, and the eye looked, she said, as if there was something white in it. In the beginning of March, the eye again began to protrude, but painlessly, and in a few days it burst, discharging a quantity of matter.

I found the left eyeball enlarged, filling up the orbit to its margin, and protruding considerably; the eyelids were swollen and livid in colour, and the conjunctiva was chemosed, pale, and flabby; the lower half of the cornea was gone; the upper, with a sharp and clean-cut edge, overlapped what appeared to be the remains of the iris; the opening into the eye was blocked up by a soft scab, on detaching which, and pressing gently, pus and discoloured vitreous humour flowed out; a probe was introduced through the opening, and moved freely about without being felt in the slightest degree; on examining microscopically what adhered to it, it was found to consist of pus and broken fibres of the lens.

The whole of the left side of the face supplied by the fifth nerve was insensible to such a degree, that she could merely tell when she was roughly touched, but felt no pain when pinched or pricked with the point of a pin; even this amount of sensibility she represented as being of recent occurrence; and on the forehead she had no feeling whatever. The anæsthesia affected equally the nostril, inside of the cheek, gums, roof of the mouth, and tongue of the left side. The skin of the upper lip, at the entrance of the nostril, was raw and excoriated, apparently by the thin mucous secretion which trickled over it.

The features were drawn to the left side decidedly, but not to a great extent; she said that a considerable improvement had taken place in this respect. She could not close the right eye; when she attempted to do so, the eye rolled upwards and inwards, so as to conceal the cornea, while rather more than a quarter of an inch of the sclerotica remained exposed. Neither could she close the lips, from the left corner of which, owing to its being rather more depressed than the other, there was a constant dribbling of saliva. The left temporal and masseter muscles remained perfectly passive during mastication; on the right side they acted in the usual manner. She was unable to use the left side of the

mouth in eating. The food accumulated between the teeth and the cheek, and remained there till it was pressed out purposely; and as, from the absence of sensibility of the parts, she was unconscious of its presence, it was sometimes allowed to remain until it became offensive. There was no accumulation of food in the right cheek in eating. The left side of the tongue was much atrophied, being not more than half the size of the right; it was protruded towards the left corner of the mouth. Neither the uvula nor the soft palate appeared to be implicated in any way.

As the patient unexpectedly discontinued her attendance at the hospital, the opportunity was lost of examining minutely into the state of the senses of taste and smell; the former, she said, was completely gone on the left side, except towards the back of the mouth, and as she was in no way prompted, this answer may be considered as satisfactory to a certain extent; the sense of smell, though blunted on the left side, was never altogether lost. The hearing of the left ear was not at all impaired, and this was the more evident, as she had been completely deaf on the right side for several years. Her speech was very indistinct, apparently from her inability to articulate the labial sounds, owing to the paralyzed state of the lips.

I had another opportunity of seeing her very recently. The eyeball had shrunk into a small stump, marked by the action of the muscles; she had severe neuralgic pain in it some time ago, which lasted for about ten days; but, with this exception, it has been free from uneasiness. The sensibility of the face is slowly returning in parts; on the eyelids it is perfect; on the lower part of the cheek it is little inferior to that of the right side; on the forehead and side of the nose it is not at all improved. The skin of the upper lip is still excoriated and slightly ulcerated; the sensibility of the nostril is much improved, and the sense of smell is nearly as acute as on the other side. There has been no change in the state of the mouth, except that the gums are somewhat swollen and unhealthy, and the left side of the tongue has, if anything, slightly increased in size. The sense of taste was more carefully inquired into, and her former statement found to be correct. The features are now quite straight, but the lips and the right eyelids remain paralyzed as formerly; when she attempts to close the former, the muscles of the chin, on the left side, are seen quivering under the skin. When she uses the jaws in mastication, the temporal and masseter muscles of the left side are now felt to swell under the finger, but less firmly than those of the right side, and not quite synchronously with them; the muscles of the right side act first, and then, after an appreciable interval, those of the left.

The suddenness with which the paralysis occurred, and the fact that the nerves of both sides were affected simultaneously, render it probable that the cause was an effusion of blood at the base of the brain. The situation of the lesion, whatever may have been its nature, is accurately pointed out by the symptoms which it produced, which indicate that both roots of the fifth, and the hypoglossal nerve of the left, and the seventh nerve of the right side, were the parts implicated. The complete paralysis of the orbicularis oris might at first sight lead to the idea that the seventh nerve of the left side was also involved; but, as this was the only muscle under its control whose power was at all impaired, some simpler explanation will probably occur to those who are conversant with nervous pathology.

Of the various points of physiological and pathological interest which this case presents, the most remarkable is the occurrence of the destructive inflammation of the eyeball simultaneously with the first appearance of the facial anæsthesia. In the well-known experiments of Magendie, which have since been carefully repeated by Valentin, complete division of the fifth nerve within the cranium, in rabbits, was followed by inflammation of the eye within twenty-four hours; but, in the human subject, where the anæsthesia is the effect of disease, the interval is much longer; in some instances, even when the paralysis of the nerve is complete, the eye remains totally unaffected; in others, the immunity lasts for many months, and it is very rarely that the interval is less than several weeks. These remarkable differences, which are as yet wholly unexplained, have led several eminent physiologists to maintain that the disease

of the nerve exerts no direct influence in producing the inflammation of the eye, but that the organ suffers secondarily from the irritation of dust or other foreign particles, the presence of which is unfelt, from the loss of common sensibility; or which, as Sir Charles Bell suggested, may remain unremoved, owing to concurrent paralysis of the eyelids. The fallacy of the latter suggestion is strikingly shown by the case under consideration, in which the fifth and seventh nerves were paralyzed upon opposite sides, the right eye escaping all injury, though its eyelids could not be closed, while the left, that on the side where the fifth nerve was diseased, was destroyed. In an able paper, in the twenty-eighth volume of the *Medico-Chirurgical Transactions*, Mr. Dixon has adduced much valuable evidence in favour of the correctness of Magendie's conclusions; still, in every case which has hitherto been recorded, so far as I am aware, the long interval which has elapsed before the eye was affected, has afforded some countenance to the opposite opinion. So far as a single case bears any weight, the one now reported appears conclusive; for, taken in connection with what has been already observed, it is impossible to consider the inflammation of the eye as merely a coincidence, and the rapidity with which it occurred excludes the possibility of its having been occasioned by any external irritation.

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33. *Complete Amaurosis following Retinitis, successfully treated with Strychnia.*—By DANIEL GRIFFIN, M. D. John O'Donnell, a labourer, aged 62, was received into the County of Limerick Infirmary on the 11th of November, 1852, perfectly blind, but otherwise in good health. He could hardly perceive the strongest light. He said he was but five weeks ill. The complaint began with pain over the brows and in the top of the head, attended with vertigo, so severe as nearly to render him incapable of walking. He had neither been bled nor leeched, and had taken scarcely any medicine; indeed, very little seemed to have been done for him except blistering the occiput and vertex about a fortnight after the first accession of the attack; and to this he attributed his blindness, as his sight had, he says, remained unimpaired up to that time. The iris is sluggish in its movements, but unchanged in colour, and shows no depositions on its surface; the pupils of both eyes are perfectly black, and moderately dilated; there is a zone of red vessels round the cornea, extending a considerable distance from its edge, and the conjunctiva is slightly injected throughout; he stands with that staring, vacant look which betokens the absence of all perception of external objects; there is considerable vertigo still, but the pain is abated.

Eight leeches were applied to each temple. He was ordered blue pill, hydrargyrum cum cretâ, nitre, and Dover's powder, in moderate doses, three times a day. On November the 15th, four days after his admission, the leeches were repeated as before, and he was directed to persevere in the use of the powders, which on the 29th had produced so little effect that they were ordered to be taken at shorter intervals.

December 1. There is scarcely any appearance of mercurialization, and the only change observable in the eyes is the removal of the increased vascularity, and their return, in this respect, to a healthy aspect; there is not, however, the least sign of returning vision, even hardly so much as the perception of light. As I rather distrusted his statement regarding the length of time he was ill—many persons of his class seeking admission to the infirmary merely for the sake of temporary support—moreover, as I suspected, from the severity of the pain and vertigo, that the affection was associated with organic disease of the brain, I was very much in despair as to his chances, and was about to dismiss him as incurable. On my visit next day, however, he told me he thought there was some slight amendment; I therefore directed him to remain, and ordered the following pills, one to be taken every night: Strychnia, one grain; extract of gentian, six grains; to be divided into twelve pills; the powders to be omitted. 9th. A decided and considerable improvement; the pills to be continued. 13th. Rather stationary since last report; to take a pill night and morning. 27th. The improvement goes on rapidly. He can see all objects around him with considerable distinctness, and walks much more steadily. January 13, 1853. Very much improved. 26th. The right eye is perfectly

restored, and the left nearly so. He says he can read his prayer-book quite as well as he ever did. Dismissed.

I observed, during the progress of this case, that the retina recovered its functions, not by a gradual amendment extending over its whole surface, but in parts and patches separately, almost as if these parts had no physiological connection. It was singular to observe the odd attitudes he threw himself into, as the improvement went on, to find out those parts of the membrane with which he could see when an object was placed before him, such as the hand or the finger. In all cases which have their origin in the inflammatory affection, it would seem to be of great importance to remove all traces of inflammatory action, as well as to pay extreme attention to the general health, before the use of strychnia is commenced.

This remedy has long had a high repute in the treatment of amaurosis; yet M. Riberi says, in the cases which he has hitherto administered it, he has derived little or no benefit therefrom. He suspects, however, that in some of these he has not persevered long enough; and remarks that the successful cases hitherto on record seem to indicate its use in incomplete rather than in complete amaurosis. I believe he always used the endermic method; and, from some published cases of his, he seems to depend more on nux vomica than on strychnia. It is singular to observe what a small amount of this drug is sufficient for success in some instances. The whole quantity which produced a complete restoration to sight in the case of O'Donnell was somewhat short of eight grains, and this spread over a space of about eight weeks, making about one grain per week.—*Dublin Quarterly Journal Med. Sci.* Aug. 1853.

34. *Ophthalmia in Nursing Women.*—Under the name of retinitis, Dr. Mackenzie, of Glasgow, has published an excellent notice of this frequent complaint in the *Glasgow Medical Journal*, for April. The most important part of the disease is inflammation of the retina; but almost all the other tissues of the eye may be involved. We are ourselves inclined to consider it as a disease exactly analogous to the ophthalmia which is apt to occur after fever, smallpox, or even in syphilitic cases—a disease which may affect the retina *alone*, in which case it goes under the name of amaurosis, or which may spread to, or originate in, the visible tissues of the eye, when it is denominated ophthalmia. When the retina only is affected, there is no pain and no redness; but as the choroid, iris, and sclerotica participate in the diseased action, pain, redness, and various visible lesions ensue. In all cases, however, the disease is of an asthenic character; and, although requiring local depletion and the action of mercury for its cure, neither of these powerful means are to be used in excess, and both are to be accompanied by a general tonic and invigorating treatment. In the case of the ophthalmia of nursing women, it is very obvious that the child must be taken from the breast, as the very first step in the treatment. Mild mercurials, with belladonna locally applied, if the iris be affected, preceded by local bleeding proportionate to the pain of the eye and the strength of the patient, and accompanied with nutritious diet and rest of the eye, are our best means. Dr. Mackenzie advises that quinia should be given along with the mercury: our own experience would incline us to give the mercury alone in the first instance, till the gums have been moderately touched, and to follow it by the exhibition of quinia or bark. Blue pill, in combination with Dover's powder, is the preparation we commonly employ, rubbing into the temple at the same time a mixture of blue ointment and extract of belladonna, which has often a magical effect in removing the pain. After the mercurial action has been maintained for a certain time, we have great faith in the administration of iodide of potassium, which is of no use, according to our experience, while the inflammatory symptoms continue, but, as we think, very valuable as a "sorbefacient," in accelerating the absorption of the effusions to which must be attributed the amaurotic symptoms so often persisting after an attack of deep-seated inflammation of the eye. In no other kind of amaurosis have we ever seen iodide of potassium of the least use. It is quite obvious that the length of time which has elapsed since the birth of the child, is no test whether the nursing has been unduly prolonged or no. A few weeks is in some cases quite enough to impoverish the

blood, and to exhaust the nervous system. The important point is, that practitioners should bear in mind that no affection of the eyes, occurring in a nursing woman, is to be trifled with; though merely conjunctival at first, it may soon lapse into deeper-seated disease; and slight dimness of vision may, if neglected, rapidly pass into confirmed amaurosis.

We may notice here a paper by Dr. GRIFFIN, of Limerick, in the last number of the *Dublin Quarterly Journal of Medical Science*, on "treatment as a test of pathological conditions;" one of his "glaring instances" being a case of iritis, in which the aggravation of all the symptoms under mercurial treatment, led him to give that up, and resort to the use of turpentine, which rapidly cured the patient. In another instance, a feeble infant, labouring under bronchitis, being nearly killed by the application of leeches, was soon restored by stimuli! Now we humbly submit, that here we have merely the old fact that our art is eminently tentative, and that we often fail in determining, before seeing the effects, what is the best plan of treatment. But surely this is from the imperfection of our diagnosis, and our want of skill only, and is "a test" of our own deficiency, as well as of our patient's "condition."

We mention this here, because in no department more often than in eye diseases, do we find lesions and objective symptoms so very nearly alike, and yet depending on pathological conditions the most opposite, and of consequence demanding the most diverse means of cure.—*Ibid.*

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35. *Surgical removal of Opacities from the Cornea.*—M. SZOKALSKI contributes to the *Revue Médico-Chirurgicale* a *compte-rendu* of thirty-two cases in which he has practised this operation. He performs it by scraping the cornea with a delicate knife, as we scrape paper to remove ink-marks from it. The epithelium of the cornea adheres pretty firmly in all cases, but when opaque, is often so tough that it cannot be removed at one sitting. The proper substance of the cornea is still more difficult to remove; in fact, the author considers that, when it yielded to his mode of treatment, the success was owing, not to mere mechanical abrasion, but to the process of absorption set going by the irritation which the operation produces. This sounds like common sense, and at the same time goes far to invalidate M. Szokalski's own results, and to induce the prudent surgeon to trust to gentler means of stimulating the absorptive process. These we see wonderfully efficacious in cases of which we might at first despair. Much, however, as all practical oculists know, depends upon the age of the patient. In infants, we never despair, though the cornea be apparently quite opaque, so wonderful are the cures which nature sometimes effects; and M. Szokalski's neglect to notice the age of those on whom he operated renders his results much less useful than otherwise they might have been.

Of the thirty-two eyes, the author reckons fifteen successful, and eight partially successful operations. In five cases, there was no change effected, and in four the occurrence of inflammation rendered it necessary to stop the proceedings. Of the fifteen successful cases, there were eight in which the opacity was limited to the corneal epithelium, five in which it affected the superficial layers of the cornea proper, and three in which it was deeper still, as it was in all the eight partially successful cases. Thus, taking into consideration the remarks we have already made as to the spontaneous disappearance of opacities in young subjects, and our author's own admission as to the *modus operandi* of his process in the case of the deeper albugines, we do really think that the use of stimulating collyria, etc., and *time*, would have produced results as favourable as his. Again, he states that three of his total failures were cases in which the opacity was merely nebulous and superficial; and, above all, the *accidents graves* which happened in four cases were of the most serious description. He states that, when inflammation does occur as a consequence of these proceedings, it always affects, not the conjunctiva, but the iris and other internal parts of the eye, and may terminate in complete destruction of the globe!

Our conclusion is, that the operation is not to be recommended, *except* in occasional cases, where the opacity fails to yield to judicious treatment of the ordinary kind continued for many months, where it is dense enough to render the

eye useless, where the other eye is also blind, and where the formation of an artificial pupil is out of the question.

One remark of the author we may quote—that his scraping process is much more likely to produce inflammation, when carried on near the circumference of the cornea. This is so far fortunate, since it is precisely in that part that opacity of the cornea is of least consequence.—*Assoc. Med. Journ.* June 23, 1854.

MIDWIFERY.

36. *Paralysis occurring during Gestation and in Childbed.*—The *Dublin Quarterly Journal of Medical Science* (May, 1854) contains a very interesting article on this subject, with the particulars of 84 cases collected from various sources, by Dr. FLEETWOOD CHURCHILL.

The following is a summary of these cases, with his remarks upon the more important points connected with them. The number of cases he admits, however, are too few to justify decided conclusions from them:—

"Of the 34 cases, in 22 the attack occurred during pregnancy; in 12, either during or after labour.

"In 23 cases where it is mentioned, I find that with 10 it was their first child, with 1, the second; with 4, the third; with 2, the fourth; with 3, the fifth; with 1, the sixth; with 1, the thirteenth; and 1 had several children, but the number is not specified.

"Of the 34 cases, there were 17 of complete hemiplegia, and 1 partial; 4 of paraplegia, in 2 of which only one leg was affected; 6 of facial paralysis; 5 of amaurosis, and 3 of deafness; but in some of these latter local palsies were combined with the cases of hemiplegia. Of 14 cases of hemiplegia, in which the side affected is mentioned, I find that 11 were of the right, and 3 of the left side.

"Of the 34 cases, 4 died.

"It may be well, however, to consider these cases somewhat more closely, and for that purpose they may be divided into two classes, those which occurred during pregnancy, and those which were attacked during or after labour.

"Of the 22 cases in which paralysis occurred during pregnancy, 12 were examples of hemiplegia; 1 of paraplegia, which had occurred previously; 4 of facial paralysis; 2 of amaurosis; and 3 of deafness. There is no regularity as to the period of gestation at which the seizure took place, for of 13 cases in which this is mentioned, in 1 it occurred in the second month; in 1 in the third or fourth; in 1 in the fifth; in 1 in the sixth or seventh; in 3 in the seventh; in 2 in the eighth; and in 4 in the ninth month; from which it would seem, upon the whole, that it is in the latter months that pregnant women are most liable to the attack.

"Of 19 cases, 11 appear to have been cured before or by delivery, and in 8 the disease continued for a longer or shorter time afterwards.

"Of the 20 cases, only 1 died, and in this case it is evident that death was rather owing to disease of the brain, of longer standing than the pregnancy, than to the paralysis which increased during that process; so that I do not think we can reckon it as impairing the comparatively innocuous character of these attacks during gestation.

"In 3 cases only was the paralysis preceded by convulsions. In most of the cases it does not appear that there were any premonitory symptoms, little or no headache, or any other circumstance calculated to excite apprehension until the paralysis supervened. The characteristics of the palsy resembled very closely those of similar attacks unconnected with pregnancy; the motor power was enfeebled or altogether lost; in some the sensibility was increased, diminished, or modified; but in others, I infer from the silence of the reporter, that it was little, if at all, changed from its natural condition. The intellect seems to have preserved its integrity in all the cases. A peculiarity of great interest

in many of these cases, and to which I shall revert by and by, is the presence of albumen in the urine, whenever that secretion was carefully examined.

"The second class, consisting of 12 cases, is characterized by the attack occurring during or after labour. It is remarkable that in 3 cases only (Cases 23, 24, 25) did the paralysis take place during labour, and of these, 2 were cases of convulsions; in all the others it not merely succeeded labour, but in most cases after an interval sometimes considerable: for example, in Case 23, it took place on the first day after delivery; in Case 27, two days afterwards; in Case 32, three days; in Case 34, seven days; in Cases 26, 29, 31, eight days; in Case 30, ten days; and in Case 33, a month afterwards.

"Of these 12 cases, 5 were cases of complete hemiplegia; in 1 only the arm was affected; 1 was a case of complete paraplegia; in 1 the right, and in 1 the left leg only was paralyzed; 2 were examples of amaurosis; 1 of facial paralysis; and in 3 only of the cases of hemiplegia the face participated in the attack. In Dr. Levy's very remarkable case, the paralysis of the motor power of one side was accompanied by loss of sensibility on the other. In some of the cases the sensibility was diminished, in others unaltered, but in none increased. The phenomena of the disease were not peculiar: in the majority of the cases the attack occurred generally without warning, and without any obvious cause. In 2 cases convulsions terminated in amaurosis, but in Mr. Forrest's case the paralysis preceded the convulsions, and during the latter, the paralyzed limbs shared in the convulsive movements.

"The duration of the disease varied a good deal, the paralysis gradually subsiding in most cases: in Case 22, after several days; in Case 23, in six weeks; Case 31 recovered the use of the arm in a fortnight, but vision remained imperfect for some months; in Case 34, in a month; in Cases 28, 29, in two months; Case 22 recovered the power of walking in two months, but was then attacked by another disease which proved fatal; Case 26 left the hospital without improvement.

"In 3 cases death occurred: in Case 32, on the fourteenth day, and in Case 34, on the twenty-fourth day after the paralytic seizure. Dr. Ley does not mention on what day his patient died.

"I have already alluded to the fact that in most of the cases the attack occurred without warning, and without apparent *cause*. Some cause there must be, of course; but it is much easier, in most cases, to say what it is not than what it is. For example, in none of these examples except one, did it appear to depend upon any external influence—upon cold, exposure, violence, &c.—or upon mental distress; in few, if any, was there evidence of previous cerebral congestion, or disease of any other organ.

"It has been suggested that the palsy may be merely the termination of convulsions, and certainly some of these cases would seem to support this view; but if this were generally true, we should find convulsions more frequently preceding the paralysis; and, also, we should meet with more cases of convulsions terminating in paralysis. Now, in all the cases I have quoted, a large majority exhibited no convulsive movements at all, and, on the other hand, of all the cases of convulsions related by Drs. Collins, and McClintock and Hardy, there is not a single instance of such a termination; we must therefore refer both convulsions and paralysis to some common or different cause.

"I have no doubt, as Dr. Romberg has observed, that in a number of cases, especially those which occur during gestation, the palsy is due to a reflex action from some organ or structure in a morbid condition, and in which the nervous system seems to be merely the channel of transmission, offering no central disorganization. In such cases the exciting cause may possibly be some injury or morbid condition of the generative organs, or perhaps merely a transient excitement, such as that of pregnancy. It is possible, also, that some of the instances occurring during gestation ought rather to be classed under the head of hysterical paralysis, as described by Drs. Laycock and Romberg; but it is not always easy to make the distinction.

"Obstruction of the arteries has been recently shown by Professor Simpson[1]

[1] Edinburgh Monthly Journal, February, 1854.

to be an occasional occurrence in childbed, either from arteritis, a coagulum, or a detached vegetation; and a degree of paralysis may be the result: but inasmuch as the death of the limb, and ultimately of the patient, is the direct consequence of such an occurrence, the history of the cases I have quoted removes from them the suspicion of being thus caused.

"It might naturally be supposed that the stress and exertions during labour, which give rise to such great congestion of the face and head by also occasioning congestion of the brain, might be considered one of the principal causes: but such a supposition is not borne out by facts, for, excluding the cases of convulsions, in only one case did the paralysis occur at the time of labour; in all the others it either supervened before labour, or subsequently, at a time when all such direct action must have ceased, and in some, after such an interval that we cannot suppose it even a remote effect of the parturient agony. On the other hand, when we remember the number of severe labours in which no such attack occurs, or compare its frequency with that of convulsions during labour, we can scarcely attribute much influence to this cause.

"Again, as we have seen, paraplegia has been attributed to severe and prolonged labour, and to the consequent mechanical pressure upon the nerves and muscles of the pelvis, and at first sight this seems an adequate and feasible explanation, and of which no one could deny the possibility; yet so far as our cases are concerned it can hardly have been so, for in all but one the labour was natural, easy, and not prolonged: in the exceptional case the patient had been delivered by the forceps; moreover, the period at which it occurred was too distant to justify our attributing it to this cause in the other cases. On the other hand, if we recollect the number of severe, prolonged, and instrumental deliveries which take place, without any such result, no example being recorded by Drs. Collins, McClintock, and Hardy, or, with the exceptions I have quoted, in any of the reports of the British and foreign hospitals, so far as I am acquainted with them, I think we must also reject this peculiarity of labour as a necessary or frequent cause.

"In two cases the attacks seem to have been connected with an anemic condition, consequent upon hemorrhage, either from the direct effect of a deficiency of the circulating fluid, or indirectly from the increased susceptibility of the nervous system, under these circumstances, to ordinary exciting causes. In another case paraplegia appeared to result from cold; but, in the majority of cases, as I have already observed, there was neither plethora nor anemia; neither exposure, want, injury, advanced age, mental distress, nor sudden shock; in short, there was no apparent cause.

"Unfortunately for the cause of science, there are very few *post-mortem* examinations on record, from which we might decide with some degree of certainty upon the nature of the affection. In all the slighter and more partial cases, life is preserved, and when death occurs in the more severe instances, permission to examine the body cannot always be obtained. Of the four fatal cases I have here detailed, two only were examined; in these, and I doubt not in the other two also, disease of the brain or its membranes existed. In Dr. Ley's case, he states that "no positive disorganization of the brain could be detected. The ventricles, however, contained more than the usual serum; and there was found, more especially opposite to the original seat of pain, thickening and increased vascularity of the membranes, with moderately firm adhesions in some parts; in others an apparently gelatinous, transparent, and colourless deposit interposed between them." In short, there appears to have been an attack of partial meningitis, and the contrast between the peculiar train of symptoms to which it gave rise, and the absence of all symptoms except the palsy in Dr. Duke's case, is very interesting, when we remember the remarkable disorganization we discovered in the latter case.

"Now in these cases we may fairly assume that the palsy and death itself were the result of the disease of the brain and its membranes, but to what are we to attribute the slighter and more numerous cases? Do they not appear to belong to the class described by Dr. Abercrombie, as 'depending upon a cause which is of a temporary nature, and capable of being speedily and entirely removed?'

"What is this temporary cause, producing so serious a disturbance, and yet scarcely, if at all, endangering life? May it be the one to which Dr. Latham refers, as observed 'in those convulsions and apoplexies which appear and disappear, the chief circumstance which attracts our attention being albuminous urine?' At any rate, it deserves our careful attention. Of the fact of the concurrence of albuminuria with certain affections of the nervous system during pregnancy and childbed, there can be no doubt whatever. Both Drs. Lever and Simpson have detected it in cases of convulsions during pregnancy and labour: the former observes: 'I have carefully examined the urine in every case of puerperal convulsions that has since come under my notice, both in the Lying-in Charity of Guy's Hospital and in private practice, and in every case but one the urine has been found to be albuminous at the time of the convulsions.' 'I have further investigated the condition of the urine in upwards of fifty women, from whom the secretion has been drawn during labour by the catheter, care being taken that none of the vaginal discharges were mixed with this fluid; and the result has been that in no cases have I detected albumen, except in those in which there have been convulsions, or in which symptoms have presented themselves which are readily recognized as precursors of puerperal fits.' Dr. Simpson's observations about the same time, and those of more recent observers, Sabatier, Legroux, Richelot, and others, have confirmed the conclusions of Dr. Lever as to the presence of albumen in the urine in cases of puerperal convulsions, so that no doubt now exists as to the fact, although we occasionally meet with cases of convulsions without albuminous urine, and of albuminuria without convulsions."[1]

"Now, as paralysis in some cases occurs in connection with convulsions, if not as a consequence of them, we might, not unnaturally, expect albumen in the urine of such patients, and accordingly, in a patient of Dr. Lever's, and in others, we find that it has been detected.

"But we may go a step further, and state that in cases where no convulsions have preceded the paralysis, albuminuria has been equally observed. Dr. Lever says of his cases, that in none in which he examined the urine did he ever fail to find albumen, and the great experience of Professor Simpson is in close accordance with this, as may be seen by the quotations I have given, and by the cases with which he has favoured me. This was observed also in Dr. Duke's case, where the paralysis succeeded the delivery; and in which I think there is ground for believing that the albumen had diminished at the time the urine was first examined. In all probability it would have been detected in many others, had an investigation been made.

"Thus we find that albuminuria may be a marked symptom in puerperal convulsions, whether terminating in paralysis or not; and in the palsy of pregnant and puerperal women, whether partial or complete, whether local or general; and if the observations are yet too few to draw any very positive conclusions, it is, I believe, because our attention has not been drawn to the subject. And when, in addition, we find, as Dr. Lever states, that as the albumen diminishes, the paralysis subsides, we can hardly doubt that there is some important connection between them.

"What, then, is the precise pathological significance of albuminuria? We

[1] It may be of interest to append Dr. Seyfert's conclusions on this subject. "1. Albuminuria is not an essential accompaniment of normal, healthy pregnancy. 2. The theory, ascribing albuminuria to the pressure of the enlarged uterus on the renal vessels, is inadmissible. 3. When anasarca, from Bright's disease, occurs during pregnancy, the patients are seldom attacked by eclampsia. 4. The albuminuria, in cases of eclampsia, is occasioned by the interruption of the functions of the respiration and circulation by the attack. 5. In such cases the albuminuria terminates with the attack. 6. Albuminuria is not present in all cases of eclampsia. 7. Albumen is found in large quantities in the urine of epileptics, *immediately after an attack;* but not invariably after every seizure, or in every case of the disease. 8. Provided there be no Bright's disease, this albuminuria among epileptics ceases soon after the convulsions, and only returns after the next attack."—*Edinburgh Monthly Journal*, Feb. 1854, p. 168.

may assume as established, that although it occurs in Bright's disease, *it alone is no proof of the presence of that disease;* but in the present state of our knowledge it is very difficult, perhaps impossible, to come to any very decided conclusion upon the matter. It is conceivable that an unusual, morbid, or noxious ingredient in the urine may be produced in either of three ways: 1. By simple elimination from the blood, in which it was present; 2. As the result of diseased action of the kidneys, excited either by some noxious principle in the blood, or by a morbid condition of these organs; or 3. As a new compound, the result of chemico-pathological action, which we may or may not be able to explain.

"Now, albumen in the urine cannot be placed under the latter category, as it is not a new principle, but one already existing in the blood. Nor does it come under the first, for although it is possible that it might be eliminated from the blood in which it is present, it cannot be as a noxious element, nor would this simple elimination account for the condition of the kidneys or for the concomitant symptoms. So that it would appear this secretion of albumen must be owing to some disordered action of the kidneys, excited by some morbid element, in kind or degree, which they are endeavouring to separate from the blood. This seems at least to be the opinion of a high authority, Dr. George Johnson, of London, who, in describing acute desquamative nephritis, in which albumen is so largely secreted, observes; 'that all the changes of structure commence in the secreting cells of the gland, and are the result of an effort made by the cells to eliminate from the blood some abnormal products, some materials which do not naturally enter into the composition of the renal secretion.'[1] This view is further confirmed by a *post-mortem* examination into the state of the kidneys themselves in albuminuria. Dr. Handfeld Jones, in a recent paper, has described three varieties: 'The first is the condition of engorgement, such as is seen in those who die in the early stages of acute anasarca, or in that of dropsy succeeding scarlatina. The organ is enlarged, dripping with blood in every part; its tissue not destroyed, but many of the tubes are seen, under the microscope, to contain coagula of exuded fibrin, entangling blood-globules, and more or less of epithelium.' 'The second form of diseased renal structure is that of the large, heavy, often mottled and pale kidney. In this there is no hyperemia, but rather the reverse state usually exists. The cut surface has not the appearance of healthy structure, and gives one the idea of some matter having been implanted among the natural constituents, so as to obscure them and to produce a confused aspect. The tubes are found impacted with epithelial matter, but not by any means constantly obstructed or blocked up, although they may be irregularly dilated, &c.' 'The third variety of morbid change is that so familiar to observation as the dwindled, granular kidney.'[2]

"When we consider the temporary nature of the albuminuria in many of the cases of paralysis, we need have little doubt that the condition of the kidneys answers to the first variety here described, or that of extreme congestion, and this opinion is confirmed by the examination of Case 34, in which we found a high degree of congestion, which had indeed passed into a more advanced stage. I think, therefore, that we may fairly assume the albuminuria is due to a congested state of the kidneys, and I confess I cannot but think that the explanation given by Dr. G. Johnson and others, that this congestion is excited by the effort to eliminate some noxious element from the blood, is more in accordance with our present knowledge than any other, yet I must not omit to mention that by some this congestion has been attributed to pressure of the gravid uterus upon the renal vessels. Dr. Seyfert, as we have seen, rejects this mechanical explanation, and seems to attribute the albuminuria to the eclampsia, in consequence of the interruption of the functions of respiration and circulation.

"But, if the former theory be true, what is this morbid element, morbid in kind or degree? It is very difficult to answer this question. Dr. Simpson suggests that it may be an excess of urea or some morbid quantity or quality of caseine in the blood. Dr. George Johnson's observations seem to prove that

[1] Diseases of the Kidney, p. 105. [2] Medical Times and Gazette.

in these cases, in addition to a change in the proportion of the normal constituents of the blood, of which the diminution of its albumen is one, there is always an excess of urea.

"Then it may be asked, 'to what is the effect upon the nervous system owing?' One can conceive that it may result either—1. From the continued presence of the noxious principle in the blood; or, 2. From the balance of the constituents of the blood having been destroyed; or, 3. From the diseased condition of the kidney—though to which of these we ought to attribute it, would be difficult to decide.

"But at whatever conclusion we arrive with respect to these interesting points, I am sure all will agree with me, that, taking the circumstances into consideration, it is probable the kidneys play a more important part in these paralytic affections than has been suspected, and that the subject deserves more attention than it has received. For, we find that in cases of convulsions terminating in paralysis, we may have albuminuria; in paralysis before delivery, without convulsions, we may have albuminuria; in paralysis occurring after delivery, we may have albuminuria; and further, that in the slighter cases, both the convulsions and paralysis diminish with the decrease of the albuminous secretion. Whether, therefore, the paralysis be caused by the state of the kidneys, or the renal congestion and paralysis be both the result of some morbid matter in the blood circulating through the system, it is clear that a new element may be added to those which have usually been considered as giving rise to paralysis.

"Nor is this barren theory only; but, if it be true, it has a direct bearing upon practice, inasmuch as our attention ought not to be confined to the secondary affection of the nervous system in such cases, but must be directed to the relief of the renal malady, and to the restoration of the kidneys to such a state of efficiency as may enable them to remove the morbid constituents of the blood; and for our encouragement, we have seen that a diminution of albumen in the urine is followed by mitigation and cure of the paralysis. For the latter affection, bloodletting, general when the system will bear it, or local by means of leeches or cupping; blisters, purgatives, and mercury, are the remedies usually employed; these must be modified according to the condition of the patient, the circumstances of the attack, and the duration of the disease. When much blood has been lost during labour, bloodletting must be omitted, and we must confine ourselves to counter-irritation; perhaps a series of small blisters to the neck, down the spine, or along the limb, will be the best mode of proceeding. The patient's strength must be supported judiciously by good diet, and it is quite possible that some stimulant, such as ammonia or camphor, may be necessary. When the paralysis has become chronic, strychnia or galvanism may be found useful; and I believe Dr. Stokes has found galvanic acupuncturation very beneficial in facial paralysis.

"The renal disorder should never be treated by diuretics, but by external irritants, such as mustard poultices, or rubefacient liniments to the loins, and internally by diaphoretics, as suggested by Dr. Osborne[1] of this city, and when more chronic, by gallic acid, iron, &c."

37. *Inversio Uteri taking place during Labour.*—Dr. George Johnston, submitted to the Dublin Obstetrical Society, a case of this which came under his notice while assistant in the Dublin Lying-in Hospital.

Esther Page, aged 19, a thin delicate-looking woman, of fair complexion, was delivered of her first child, a healthy girl, on the 31st of July, 1851. Her labour so far was easy, and of about six hours' duration. The gentleman in attendance, after having tied and separated the funis, had maintained the contraction of the uterus with the hand above the fundus—in accordance with the usual practice of the hospital—for about a quarter of an hour, when finding a tendency to "draining," he increased his pressure; but, as he said, not nearly to the extent it had been, on frequent occasions, found necessary to employ, in order to assist in the expulsion of the placenta, or restrain hemor-

[1] On the Nature and Treatment of Dropsies, &c. 1837.

rhage. The uterus was felt suddenly to yield and recede from his grasp, and he immediately saw it expelled from the vagina, an inverted mass, with the placenta still attached. Dr. Johnston was at once sent for; on his arrival he found the woman pallid, exceedingly anxious, complaining of considerable pain, and a sensation of sinking; the pulse was weak, indeed scarcely to be felt.

Examination proved the uterus to be inverted with the placenta attached to its fundus; the funis was of the ordinary length, and there was then no hemorrhage. Recollecting that the lapse of every minute was of consequence, Dr. Johnston proceeded to replace it at once, which he accomplished in the following way: He first detached the placenta—a matter of no difficulty, there being no morbid adhesion—and he was pleased to find that after it was separated, no hemorrhage followed, owing, he considered, to the constriction the vessels underwent at the cervix; he then restored the cervical portion of the inverted organ, which was easily returned within the vagina, and reinverted as far as the body; but it was some time (five to seven minutes) before he could reduce the fundus, which required the fingers to be held in a flexed condition against it, while he made counter-pressure with the left hand above the pubis. Some wine had been given to the patient to relieve the sensation of exhaustion, but it was not till the uterus had been restored to its natural state that she could be persuaded her immediate dissolution was not close at hand; ergot was afterwards administered, and she was kept longer in the horizontal position than ordinarily. Milk was secreted on the third day; she made a perfect recovery, and was discharged quite well.

Dr. George Johnston then proceeded to remark as follows:—

"That inversion of the uterus very rarely takes place, is universally allowed by all obstetric writers.

"Denman, Rigby, and Churchill, all agree in believing it to be an accident of 'rare occurrence.'

"Ashwell says: 'It *is most rare*,' and in proof states that, in more than 8,000 labours occurring in Guy's Hospital, and upwards of 1,600 in another charity, there was not a single instance of it; if others were wanting, the records of our own hospital show (according to Hardy and McClintock) that, during the masterships of Doctors Clarke, Labatt, Collins, Kennedy, and Johnson, the number of deliveries amounting to 75,911, not one case of this description occurred; and I may add that, subsequent to that period, for three years and eight months of the management of the present master, Dr. Shekleton, 7,336 patients were delivered previous to the occurrence of the one I have just related, so that in 83,247 deliveries, there was not a single instance of its being met with.

"But, 'inversio uteri,' although rare, is nevertheless considered to be one of the most formidable complications that affect the lying-in patient, being attended with very alarming symptoms, and threatening the most serious consequences. For instance, the patient is found complaining of excruciating pain, with a sense of sinking and extreme exhaustion; an almost imperceptible pulse; a countenance expressive of great anxiety and collapse, together with nausea, vomiting, and sometimes hemorrhage and convulsions; in fact, the symptoms in a case of inversion resemble in many points those attendant on rupture of the uterus; but this former accident can be generally distinguished from rupture by the period at which it takes place, viz., after the birth of the child; by the sudden recession of the round, circumscribed tumour, if the uterus be grasped after the delivery of the child, or its absence, when searched for. All doubt is set at rest by finding a globular fleshy body occupying the os uteri, if the inversion be partial; or protruding quite through the os and vulva, if it be complete; thus forming, as it were, a sac lined by peritoneum, a diverticulum from or extension of the abdominal cavity, filled by the small intestines. It is this sudden evacuation of so large a portion of the contents of the abdomen, which produces that severe shock on the nervous system, from which there is great danger the patient may not rally, death in more than one instance having been the result.

"This accident has been attributed to various causes, viz:—

"1. To undue pulling at the funis for the purpose of extracting the placenta before it is completely separated.

"2. To an over-amount of pressure with the hand on the fundus uteri.

"3. To too rapid delivery, especially if the woman be standing at the moment of the quick expulsion of the child.

"4. To the cord being too short, or twisted round the neck or body of the child.

"5. To violent straining during the last pains; violent efforts, as coughing, vomiting, sneezing, or by sudden attempts to rise in bed, by which the abdominal muscles are put into violent action.

"6. It may take place spontaneously.

"That it may be produced by the first two causes, no doubt at all exists, particularly the first—'if,' as Dr. Ramsbotham observes, 'the placenta be adherent to the fundus; if the adhesion be strong; if the funis does not give way to the force applied to it; and if the uterus be flaccid, and has not contracted round the mass.' And with regard to the second, Rigby goes so far as to say: 'If immediately after delivery, especially where the uterus has been suddenly emptied of its contents, any *force* be applied to the fundus, it may easily be pushed down into the cavity.'

"That it may result from the occurrence of too rapid delivery, especially while the woman is in the erect posture, is possible: but such cases of delivery have frequently come under the notice of those connected with the Dublin Lying-in Hospital, and yet no such accident has taken place.

"As to shortness of the cord, or the twisting of it round the neck of the child, numerous instances of the kind could be brought forward where there was no attempt at inversion.

"That violent strainings or efforts, or sudden attempts to rise in bed, have been the cause of this accident, instances are given by Cazeaux: one, where complete inversion took place nine hours after delivery, from rising to the night-chair; another, where complete inversion resulted twelve days after delivery, in consequence of straining efforts at stool. Lastly, that it occurs spontaneously; an instance is given in the author's notes on Denman, where, after the funis had been divided, and the doctor engaged with the child, not the slightest extension having been made on the cord—'in fact, it had not been touched by the hand'—the uterus was inverted, with the placenta attached.

"Dr. Rigby, in giving a case of this description, says: 'The descent was so rapid and forcible through the os externum, that it would have been quite impossible to have resisted the unnatural action by which the organ was carried down;' and Ruysch saw it take place after the expulsion of the placenta, although delivery had occurred in the most favourable way.

"The predisposing causes of inversion are, according to Radford, owing to atony of the uterus, or active constriction of one part, with an atonic condition of another.

"Dr. Tyler Smith says it depends upon an irregularly active and a very unusual condition of the uterus, by which the fundus is first depressed, then carried downwards by the annular contraction of the organ, and finally completely inverted.

"Cazeaux states, that the uterus, being for a moment in a condition of inertia, the pressure or weight of the intestinal mass upon the fundus may depress it like the bottom of a bottle; and when the placenta is inserted directly on the superior part of the uterus, its weight alone may (in a case of complete inertia) draw down the fundus. This, he says, however, generally corrects itself by the contraction of the viscus; but if (this depression not being perceived) traction is made on the cord, or pressure on the fundus, it may be greatly increased, and converted into complete inversion: this was probably the cause in the case just narrated.

"With regard to the treatment of this accident, it is universally agreed upon, that the immediate reduction of the inversion is the best practice, when 'we find'—as Denman remarks—'no difficulty, or very little, in restoring the uterus to its perfectly proper situation.' But difference of opinion exists as to

the mode of acting when the placenta is still adherent to the uterus; some recommending the returning of the tumour before detaching it. For example, Newenham advises 'returning the uterus first, and exciting it to throw off the placenta afterwards in the usual way; 'which,' he says, 'will have good effect in bringing on that regular and natural contraction which is the hope of the practitioner and the safety of patient: that the removal of the placenta first, in order to diminish the bulk of the inverted fundus, cannot possibly be attended with any beneficial consequences, whilst the irritation induced by such a proceeding will necessarily tend to bring on those pressing-down efforts which would present a material obstacle to its reduction, and would increase the hemorrhage at a period when every ounce of blood is of infinite importance.' Denman goes half way with the above maxim, for he says: 'If the placenta be partly separated, it will be proper to finish the separation before we attempt to replace the uterus; but if the placenta should wholly adhere, it will be better to replace the uterus before we endeavour to separate the placenta.' And his reason he asserts to be: 'That while we are separating the placenta, the cervix of the uterus is speedily contracting, and the difficulty of replacing it increasing, which is a far greater evil than a retained placenta.'

"Now, though Dr. Newenham's and Dr. Denman's ideas may be perfectly correct in cases where the uterus is partially inverted, that is, where the fundus only is displaced, nevertheless, in cases of complete inversion, although we find Dr. Williams saying, that 'the organ, with the placenta still adhering, was promptly returned to its proper situation,' Dr. Merriman states, that the mass of the placenta was the chief cause of the difficulty in a case which he has related. 'I tried,' says he, 'to effect the reduction without removing the placenta, but could by no possibility accomplish it till I had first separated the placenta; this being effected, I succeeded to my entire satisfaction in re-inverting the fundus.'

"For my part I am of opinion that, in cases of complete inversion, it is the best and simplest treatment to detach the placenta in the first instance (which is easily, and in a very short time accomplished), and then to reinvert the uterus, a matter also of but little difficulty, as it is then much diminished in bulk. The objection that Denman raises to this practice is not, I conceive, a very strong one; for 'the time' it takes to peel off a placenta, under these circumstances, would be far less than that which would be occupied in trying to force so large a body as the placenta with the uterus back through the vagina and os. As to the occurrence of an increase of hemorrhage from thus proceeding, I consider there is no danger, or at most but trifling. Even Dr. Denman, in the history of the case, where he pursued this line of treatment, informs us, that 'the hemorrhage was not profuse,' and 'that regular and natural contraction,' which, as Dr. Newenham observes, 'is the hope of the practitioner, and the safety of the patient,' is, I think, more likely to be produced when the uterus is empty, than when it still contains the foreign body, which its efforts to expel had been the cause of the production of that very displacement we are called upon to remedy. The placenta having been detached, we proceed to reduce the tumor by grasping the body of the organ, and pushing it with the vagina in the axis of that passage, continuing this pressure till we meet the os, when the first obstruction presents itself; but, by the steady maintenance of our pressure, and by moulding, if I may so express myself, gradually and progressively, first, the cervix, and then the body through the os, we complete the reduction by pushing, lastly, the fundus upwards.

"With regard to the manner in which the last stage of the operation is completed, one recommends the fingers to be held in the form of a cone, another spread out at equal distances; but I held them in a flexed condition, pressed against the displaced fundus, and continued steady pressure till that portion was righted; and, in fine, held the hand in the cavity of the uterus, till it was expelled by contraction.

"In the above case I made counter-pressure over the pubis, finding the uterus yield so much from my reach, owing to the relaxed state of the vagina; and my object in administering the ergot was to secure uniform construction.

"It is said by Cazeaux that inversion of the uterus having once taken place

in a labour, there is a tendency to its recurrence at a subsequent delivery, and I regret that in this instance an opportunity has not been afforded of testing this fact."—*Dublin Quart. Journ. Med. Sci.* Feb. 1854.

38. *Phlegmasia Dolens in the Non-Puerperal State.*—Dr. McClintock read to the Dublin Obstetrical Society (April 30, 1853), a communication upon phlegmasia dolens, as a disease of women *not* in a puerperal state. After a brief historical sketch of the notices by Puzos, Willan, Lee, Copeland, Meigs, and others, of this disease under the circumstances just mentioned, he proceeded to narrate an instance of the kind that had fallen within his own knowledge.

The case differed from those related by any of the above authors in its purely idiopathic character. The possibility of any chronic uterine disease having existed was not admissible, neither had there been anything like symptoms of metritis, nor yet was the crural attack preceded by *sudden* suppression of the catamenia.

The subject of the case was a young lady, aged eighteen, who had been for some time under treatment for scanty menstruation and symptoms of incipient chlorosis, consequent upon a change of residence from the country to town. One evening in autumn, whilst the menses were present—though, as usual, in small quantity—she stood for nearly two hours together on damp grass. On the following morning she felt the right leg rather stiff and painful; towards evening it grew worse, and began to swell. Two or three days passed over before alarm was excited, or any treatment adopted, and by this time the symptoms had become fully developed. The pulse was 120; the right leg was swollen, tense, and free from discoloration; no pitting on pressure; not much pain complained of, unless the limb was moved or handled; marked tenderness in the groin, over the femoral vessels. The line of treatment pursued was the same as that usually employed for phlegmasia dolens, and consisted in the application of leeches over the femoral vessels in Scarpa's space, constant stuping of the entire limb, absolute rest, and low diet. Under this management, the acute symptoms subsided in the course of a week. One relapse took place, which necessitated a recourse to the antiphlogistic treatment, and considerably retarded her convalescence: after this was subdued, some stiffness, and enlargement of the leg from the knees down, still remained, and continued for very many months, in spite of bandaging, frictions, &c. These symptoms were always increased towards evening, or after much walking or standing. Nearly eight months elapsed before the limb had so far recovered its former state and condition that she could use it in the ordinary movements of progression without feeling any pain or inconvenience.

In conclusion the author remarked, that in this case the existence of chlorosis, which is universally held to be a blood disease, tended to confirm the views of Dr. M'Kenzie, in regard to the etiology of phlegmasia dolens, that physician being of opinion that vitiation of the blood has much to do with its production. —*Dublin Quarterly Journ. Med. Science*, Nov. 1853.

39. *Duration of Pregnancy.*—Dr. J. Mathews Duncan, in an interesting paper (*Monthly Journ. Med. Sci.* March, 1854), gives the following conclusions:—

1. That the interval between conception and parturition (the real duration of pregnancy) has not been exactly ascertained in any case.
2. That the average interval between insemination and parturition (commonly called the duration of pregnancy) is 275 days.
3. That the average interval between the end of menstruation and parturition is 278 days.
4. That the intervals between insemination and parturition, and between menstruation and parturition, have no standard length, but vary within certain limits.
5. That while absolute proof of the prolongation of real pregnancy beyond its usual limits is still deficient, yet there is evidence to establish the probability that it may be protracted beyond such limits to the extent of three or even four weeks.

40. *Cæsarian Section with Good Result both for Mother and Child.* By Dr. Hamer, of Ommerschans.—A primipara, aged thirty, who had suffered in her youth from rachitis, experienced her first pains February 2, 1853, at the normal term of pregnancy. At 8 P. M. the membranes burst; but the head did not descend into the pelvis, and the pains ceased. It was therefore thought desirable to turn. The hand, however, encountered an insuperable obstacle in the promontory of the sacrum, between which and the pubes there were only two and a half inches. At 11 P. M., the Cæsarian operation was performed. The first incision was made along the linea alba; the second incision into the uterus extended into the placenta, from which much blood flowed; but the extraction of both child and after-birth occupied but a few seconds, after which the uterus contracted firmly and sufficiently. The child, a boy, survived. The abdominal wound in the mother was united by sutures, and a proper bandage was applied. Upon the occurrence of pain towards evening, in the hypogastric region, a catheter was introduced into the bladder, and some urine drawn off; and this was often repeated, to the patient's great relief. On February 4, there was a tympanitic state of the abdomen; the tongue was dry; pulse frequent; lochial discharge normal. Ordered an enema; the application of ice. The day following there was severe pain in the abdomen; vomiting of green matter, by which three sutures gave way, and the gaping uterus was exposed at the bottom of a deep wound. Twenty leeches were applied, and the wound now reunited by plaster. A tube was introduced into the rectum, by which much gas was drawn off, and the tympanitis was diminished. A quarter of a grain of morphia was then administered, and the patient obtained some sleep. To support the abdomen, two pieces of wood, to which silk threads were attached, were rolled in long pieces of plaster. The strips of plaster were then applied from the lumbar region in such a fashion that the wooden staffs lay near the mesial line longitudinally on either side of the wound; the silken threads were then tied, and the aperture was firmly closed. In six weeks she was well.—*Med. Times and Gaz.* Aug. 12, from *Schmidt's Jahrb.* bd. 83.

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41. *Case of Pregnancy in a Rudimentary Horn of the Uterus, with probable Advance of the Ovum from the right Ovary into the left Horn of the Uterus.*—In his *Handbook of Pathological Anatomy*, Prof. Rokitanski describes an unique preparation, in the Viennese anatomical collection, exhibiting a pregnancy in a rudimentary uterine horn. The example of Professor Scanzoni is an interesting addition to this extraordinary class of cases.

The woman generally enjoyed good health, and was regularly menstruated. She was married in her 28th year, had a miscarriage of twins five months afterwards, and subsequently bore three children, which still live. In her last two pregnancies she tried by violent exercise to induce premature labour. During the first half of all her pregnancies she suffered much from vomiting, toothache, heartburn, and œdema of the lower extremities.

In July, 1852, she conceived for a fifth time, and again used every means to induce premature labour. No disorder of general health occurred in this pregnancy. On the 21st Nov. she had a quarrel with her husband. The same evening, and also on the next, she complained of slight colicky pains in the left hypogastric region, which, however, did not prevent her from going about. But she soon became very weak, and had to be carried to bed. This was at ten o'clock, forenoon. At midnight, Dr. Scanzoni visited her and diagnosed an extra-uterine pregnancy, with profuse internal hemorrhage, in consequence of the rupture of the containing sac. She died almost immediately afterwards.

At the *post-mortem* examination, there was found in the lower half of the abdomen a great quantity of variously altered blood. After its removal, there was observed on the left side a round swelling of about the diameter of 3½ inches, with a laceration on its outer border. In it were contained the membranes and placenta of the fœtus, along with the coagula of blood. The cord was six inches long. The corpus luteum was in the right ovary. Both Rokitansky and Scanzoni, on first viewing their cases, thought they had to do with tubal pregnancy, and only on further examination discovered the true nature

of the cases. Such cases are described by Rokitansky as an intermediate link between uterine and tubal pregnancy.

Scanzoni's case is interesting physiologically, as belonging to a series where the corpus luteum is formed on the ovary of the side not corresponding to the uterine horn or tube which is pregnant. Such cases at once suggest the idea that the ovum must have performed a long journey from the ovary of the one side, through the uterus, and into the passages belonging to quite the other side. Scanzoni states that many analogous observations have been made in the lower animals, but he is mistaken in supposing that his own is the first in the human subject.—*Edinburgh Med. and Surg. Journ.* Jan. 1854, from *Verhandlungen der Phys. Med.* 1853.

HYGIENE.

42. *Lead Cisterns and Pipes in a Sanitary point of View.* By ROBERT DUNDAS THOMPSON, M. D.—Having been consulted on the subject of the supply of water to Glasgow, Gorbals, Dumfries, Kilmarnock, Sterling, London, Newcastle, Swindon, Liverpool, &c. &c., and very extensively on the sanitary condition of waters, the subject of the action of water on lead has for many years been familiar to me, and I have had considerable opportunities of meeting with cases where paralysis had been produced by the action of lead on the human system. As far as I can ascertain from the experience of hospitals, the occurrence of disease from the corrosion of lead pipes is an exceptional circumstance, if it has ever occurred in these institutions, the general cause of affections from lead being occupation in white lead manufactories.

Waters, from whatever source, appear to act on a freshly polished surface of lead. Thus, I have found the water taken from the Thames, Clyde, Gorbals waterworks, Kypes River, Paisley waterworks, Givel River, sources in the neighbourhood of Glasgow, to act on lead; the greatest amount of saline matter in these specimens being in that of the Thames, which contains about twenty-two grains of solid residue in the imperial gallon, while the Givel contains only about seven grains. Again, I find the water from a well at St. Thomas's Hospital London, to act very sensibly on a fresh surface of lead, although the solid constituents in the gallon amount to about one hundred grains. This experiment is further corroborated by a circumstance in reference to a well, respecting which I was consulted several years ago in Glasgow. The wells of Glasgow, like all those of large cities, are known to be impure. They contain from fifteen to one hundred grains of salts in the imperial gallon, and yet, on one occasion, water was brought to me which had been pumped through a new lead pipe from a well in a garden, which contained a considerable quantity of oxide of lead diffused through it. I inferred that the oxide of lead was principally in suspension, from the fact that when the water was filtered through a single paper filter, no lead could be detected in the water when it had passed the paper; and it is a well ascertained fact that water contaminated with oxide of lead, is entirely freed from it by permeating a filter of sand. All these facts relate to lead possessing a bright surface. For lead, when allowed to remain in these waters for a few days, ceases to suffer appreciable corrosion; or, if the lead be removed from the water, exposed to the air, and afterwards immersed, but an insignificant action, if any, can be detected.

Loch Katrine water I examined several years ago, when it was proposed to be introduced for the supply of Glasgow. I considered it then, as I do now, a water admirably adapted for domestic use, and I have not had my opinion in the slightest degree affected by the laboratory experiment exhibited on the committee's table, as I am convinced, from my acquaintance with the subject, that if the Loch Katrine supply had been introduced into Glasgow, nothing would have been heard of its influence on lead. I found it to contain about two grains of solid matter in the gallon, its main constituents being organic matter, common salt, sulphate and carbonate of lime.

When lead with a clean, bright surface is introduced into it, the lead is rapidly acted on, and white scales of oxide fall to the bottom of the vessel in which the experiment is made. When such water, with the suspended oxide of lead, is passed through a double filter of paper, the oxide is detained on the filter, and little or none seems dissolved in the water which passes through the paper. Hence it would happen, that should any corrosion occur on the first use of new lead cisterns, the insoluble oxide will be deposited at the bottom of the cistern, and will only intermingle, in a trifling degree, with the contents of the cistern; while in old cisterns, or after the new cisterns have become tarnished, no action will occur. But to prevent any corrosion on first using the cisterns or pipes, the plan sometimes adopted at Tunbridge Wells might be had recourse to, of brushing over the fresh surfaces with a coating of lime. No description of water could be purer or better fitted for a beverage, or for culinary purposes, than the water supplied by lead pipes to the Trossach's Hotel, at Loch Katrine, where I lately tasted it when inspecting the experiments made under the charge of the engineer, at the outfall of Loch Katrine.

To set the objections at rest which have been urged against the use of Loch Katrine water, I may detail an experiment on a sufficiently large scale of a parallel nature, which has been in action for forty years. About 1814, a plumber at Tunbridge Wells introduced, at his own risk, a spring of water, by means of lead pipes and lead cisterns, into the houses of that place. A similar objection was taken to its use as on the present occasion. Traces of lead were even detected in some places in that portion of the water in immediate contact with the new lead cisterns, but none in the body of the water, or in the water discharged from the cisterns. Specimens of this water were sent to London in 1815, and tested by Dr. Thomas Thomson, without his being able to detect a trace of lead. I have a letter in the handwriting of the late Dr. Wollaston, dated 27th of December, 1815, in which he states that he could detect no lead in water sent to London from Tunbridge Wells. Traces were occasionally detected in the new cisterns, and, as I was assured by the late Dr. Thomson, only on the margins in contact with the lead, the largest quantity obtained being one grain in twenty gallons. Yet, from these incidental results, the water supplied to the village was condemned by the opponents of the scheme as *poisonous*. But the water still continues in use; the village has increased to a large town of 10,000 inhabitants; it is a popular place of resort for invalids; and after careful inquiry, I have not been able to discover among its residents even a suspicion of its contamination by lead. I examined the company's engineer, who was employed at the original works, and laid down many of the lead pipes and cisterns in his capacity, at that time, of plumber, and persons in the town, residents of about twenty years, who assured me that they had never heard the subject of danger to water from lead pipes mooted.

When bright lead is introduced into this water, it is acted on immediately. It contains between three and four grains of salts to the imperial gallon, and is, upon the whole, one of the purest waters which I have examined in reference to the supply to towns. In examining the reservoir in which the water is retained for the supply of the town, I observed a perforated plate of lead, through which the water passed to the iron conduit pipe, which was covered with a coating of oxide of lead, precisely similar to the coating lining the lead-pipes which I have seen brought from Inverness, a town with whose water supply I am well acquainted. The plate, I was told, had been in this position for six years. The deposit was therefore insoluble, or nearly so, in the water, and acted as a protecting covering against any further action on the lead. Hence it would appear that this water, by its rapidly oxidating power on lead, furnishes with so much the greater efficiency security against further corrosion. Perhaps no stronger fact could be adduced in proof of the perfect confidence of the inhabitants of Tunbridge Wells in the sanitary quality of the water than that—of which I was assured by the engineer—the company is now paying an annual dividend of ten per cent. The original spring introduced for supplying the village is situated at about the distance of a quarter of a mile to the south, and is emitted from the northern aspect of a declivity. Many years after it was in use, from the increase in the population, the supply was found to be inadequate

to the demand, and another spring, about a mile distant in the same direction, but issuing on the southern declivity of the same ridge, was collected in a similar reservoir of brick, and pumped into the village reservoir, to mix with the waters of the first spring, which is conveyed to its destination by gravitation. The second spring I found to be more rapid and more extensive in its action on lead than the water of the first spring, which alone, as far as I could learn, had attracted any attention; and even the circumstances to which I have already referred were quite unknown to any person with whom I came in contact. Still more recently, a third spring, under different management, but of very soft water, and therefore with the corrosive qualities upon lead of the waters affording the previous supply, has been introduced into the town, with the universal approbation of the inhabitants, as far as regards its wholesomeness, purity, and softness.

Irrespective, then, of the probability that the Loch Katrine water will lose much of its corrosive power by its contact with the various strata over which it must pass in the conduit, during its flow to Glasgow, I am of decided opinion that no more permanent danger is to be apprehended, in reference to health, from the transmission of the water through lead pipes, and detention in lead cisterns, than there is in the case of other waters supplied to towns. I may add, however, that I have always recommended the substitution of iron and other materials, as water-pipes, as much as possible, for lead; and, even when lead is employed, that it should be alloyed with tin.—*Lancet*, July 29, 1854.

MEDICAL JURISPRUDENCE AND TOXICOLOGY.

43. *Poisoning by Solanin.*—An opinion has been prevalent, that certain diseased states in domestic animals arose from the deleterious action of solanin contained in potatoes, on which they had been fed. In testing the truth of this opinion, Dr. Fraas made the experiments, of which the following account is an abstract. Otto had previously found that the ripe potato, unsprouted, contained only a trace of solanin; that more of this principle was discoverable in the stalk of the plant, but the greatest quantity in the sprouts, especially the shorter ones. A similar result was obtained by Berchtold, in the spring of 1853. In 100 grammes of the green potato-sprouts, he discovered 4 milligrammes of solanin; and another time, in the very short sprouts, 12 milligrammes.

The first observations on animals were made on two pigs, to one of which were given sprouted potatoes boiled, together with the water in which they were boiled, which necessarily contained all the soluble solanin; while to the other were given the boiled potatoes without the liquor, the food being, consequently, assumed to be free from solanin. This was continued from the 15th of April to the 8th of July, without any signs of ill health appearing in either animal, notwithstanding that the outbreak of the disease in pigs was mentioned in the journals as having appeared.

The next observations were made with pure solanin, 10 grains of which were given to a pig, without the least injurious result, and the next day 20 grains were given to the same animal, with no further result than producing a diminished appetite and frequent white thin stools, the animal being quite well again the next day. On the third day, 20 grammes of acetate of solanin were given without effect.

Five grains of pure solanin were given to two dogs, the only results of which were vomiting and dilatation of the pupil. Ten grains were now given, and the œsophagus was tied; strong efforts at vomiting were made, and there was much vascular excitement, but the dose was not fatal.

The sulphate of solanin was injected into the veins of dogs. In nine dogs, death resulted from doses of from 5 to 2 grains thus administered; smaller doses only served to excite the circulation. In the case of one dog, into whose

right jugular vein 5 grains were injected, respiration suddenly became difficult, accelerated, and spasmodic, with convulsions and tetanic extension and drawing back of the head, and death occurred in seven minutes.

Two grains of solanin were injected into the right jugular vein of a horse without any result being observed. Into the jugular vein of another horse, 30 grains of sulphate of solanin were injected. The animal was suddenly attacked with severe difficulty of breathing, and exhibited a great increase in the action of the heart, with convulsions, so that he appeared to be dying. In the course of twenty minutes he appeared quite recovered.

Two grains of acetate of solanin were injected into the rectum of a rabbit. The symptoms produced were heaviness, apathy, and slowness of movement; dilatation of the pupil followed, increased activity of the circulation and respiration, and convulsions, which, however, ceased in about two hours' time. The animal now moved but little, but when it did attempt locomotion, it dragged itself along with difficulty; but no special loss of power of the hinder extremities was observed. It died in six hours.

To a second rabbit, a certain quantity of acetate of solanin was given daily by the mouth, commencing with one grain. After several days, no effect being produced, the dose was increased to 2, and after another interval, to 3 grains. Some days after this the appetite lessened, but no other alteration being observed, the dose was increased to 4 grains. At last, the animal became heavy, slow in its movements, remained lying the greater part of the day, and at last died, without paralysis or any considerable fever having been observed.

Dr. Fraas is inclined to the opinion, that the pernicious effects which often follow feeding upon raw potatoes and potato-stalks, are due to the great quantity of alkaline and earthy salts which they contain, and never to the solanin. An analysis of the potatoes used in his experiments, gave the following results: In 100 parts of dried potatoes there were 4.22 per cent. of ash; or in 100 parts of fresh potatoes, 1.17 parts of ash. 100 parts of ash contained—

Sulphuric acid	2.90
Phosphoric acid	12.37
Silicic acid	A trace.
Chlorine	4.23
Potash	52.23
Soda	A trace.
Alumina	A trace.
Oxide of soda	A trace.
Magnesia	2.41
Lime	3.68
Carbonic acid	20.18
Loss	2.0
	100.00

The potatoes contained no solanin.—*Brit. and For. Med.-Chirurg. Rev.* July, 1854, from *Archiv. für Pathol. Anat. und Physiol. und für Klinische Medicin*, Bd. vi. Heft 2.

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44. *Causes of Death from the Use of Anæsthetic Agents.*—Dr. K. King relates (*Edinburgh Med. and Surg. Journ.* Jan. 1854) a case of death occurring forty hours after the administration of chloroform, and makes the following remarks on the causes of death from the use of anæsthetic agents, which are worthy of consideration:—

"The subject of anæsthetic agents is one comparatively new to the profession; and although its rapid adoption and speedy admission as one of the recognized steps of every very important surgical operation, seem to disprove that slowness to leave the beaten track which has often been objected to against medical practitioners, it must be admitted that there still is a division of opinion as to the extent of their applicability, and the amount of danger which attends their use. In the opinion of some, whenever pain is to be inflicted, these agents may be employed, unless special circumstances, as diseased heart,

&c. contraindicate their use. Others, again, would limit their application to the severest surgical operations, considering that a certain amount of danger always attends their administration; and that therefore they are inapplicable in all cases which are usually painful, but unattended with fatal results. It is admitted by all, that deaths have not unfrequently occurred after the use of anæsthetic agents; but, according to some, the deaths actually caused by these agents are very rare, some of them being caused by accidental circumstances, and others by a neglect of those means which, if applied in time, would have obviated the fatal result. On inquiring into the mode in which death is produced, we find a remarkable discrepancy of opinion. In a very excellent paper in the September number of the *Edinburgh Monthly Journal* by Mr. Bickersteth, several cases and experiments are adduced to prove that death takes place usually, if not universally, by asphyxia. He gives as the result of his observations, that 'the respiratory movements cease before the cardiac;' and that, 'if artificial respiration be resorted to before the cardiac contractions are seriously affected, and be properly maintained for a sufficient period, the respiratory functions may be re-established.' He states it as his opinion that 'the pulse should not be taken as any guide during the administration of chloroform. It should be wholly disregarded, except under certain circumstances.' The attention should be mainly directed to the respiration; and, without denying the possibility of exceptions owing to peculiar idiosyncrasies, he considers that while the respiratory act is regularly performed, the state of anæsthesia may be produced without incurring any danger; but if the exhibition of the agent be persevered in while that state continues, syncope and asphyxia may occur simultaneously and cause sudden and irreparable death. But the fact of the pulse becoming less frequent is of no consequence—indeed is 'so constant, that by some it is supposed, and correctly supposed, that anæsthesia is not sufficiently complete for the performance of surgical operations, unless the pulse be below its standard.' Such at least is the general impression which his very admirable paper conveys to my mind; and if we can subscribe to his opinion, the induction of anæsthesia loses almost all its terrors, for death is preceded by a period of warning when, by prompt and simple measures, the fatal termination may be averted. The patient is in the condition of one seemingly drowned, but still capable of resuscitation. But unfortunately we find a different theory and different practice as decidedly and as ably recommended. In the *Lancet* of November 19, 1853, there is an abstract of a paper by M. Jobert De Lamballe, in which he says that 'the administration of chloroform should cease immediately when the beats of the heart decrease in number and force;' and again, 'the action of the heart should always be our guide; it is the best means of ascertaining the saturation of the nervous system by chloroform, and of judging how far this system has been influenced by the anæsthetic agent.' As to practice, M. Jobert 'prefers acting principally on the nervous system, rather than lose time by exciting the functions of respiration and circulation.'

"Without going at any length into this matter, I believe that there is much greater probability of asphyxia than of syncope supervening from the use of chloroform; because the muscles of respiration (and consequently the respiratory act) are governed by nerves connected with the cerebro-spinal system, which is much more readily affected by this agent than are the sympathetic nerve and its branches, which govern the cardiac movements, and which we have no reason to suppose are affected except secondarily. It is therefore natural to conclude that in most cases the failure of the respiratory act will take place before that of the heart; and that on its being noticed and the proper means adopted, time will be given to arrest the circulation of venous blood in the arteries, the continuance of which would necessarily prove fatal. At the same time, I fear we must consider it as proved that syncope may supervene without this premonitory notice. In a case recorded by Dr. Dunsmure in the November number of the *Monthly Journal*, the breathing was not observed to fail before the pulse stopped. Those 'who had an opportunity of observing the respiration, positively assert that the breathing did not cease before the pulse.' It is stated that after 'artificial respiration was had recourse to, in a

few moments he made a long inspiration, which was followed by four others at gradually lengthening intervals,' while 'no pulsation could be felt in the radial arteries.'

"An anonymous correspondent of the *Medical Times* (October 15) concludes from these facts that this was a case of 'mere syncope,' and that therefore, as a rule, 'the state of the pulse should be attended to, as betraying the first symptom of impending danger.' But it must be remembered that in such cases every inspiratory act is necessarily observed, while we have no clear evidence of the last contraction of the heart: the mere fact of the pulse at the wrist being imperceptible is no evidence that the heart had not contracted; and I have some difficulty in believing that five acts of inspiration took place without a single contraction of the heart! I believe, in this instance, that a failure of the heart's action (syncope) and a paralysis of the respiratory muscles (leading to asphyxia), occurred at the same instant—that a partial reaction took place, when most probably the observed acts of inspiration were accompanied by unobserved contractions of the heart—but that the efforts of nature were incapable of setting the machinery in motion, and that from the last natural inspiration the man's death is to be dated. The anonymous writer before referred to seems to think that this event is to be dated from the last perceived pulsation; but it would be monstrous to apply the term death to a man who was still capable of making natural respiratory efforts; and, if it were as capable of demonstration, I believe that in this case contractions of the heart would have been found taking place after the last act of inspiration. But I do not deny that deaths have occurred in which failure of the heart's action has been the prominent symptom, and that syncope has been attended by its usual symptoms of pallor of the countenance, &c. (In Dr. Dunsmure's case the face and heart are said to have been a good deal congested.) Two cases are mentioned in the Dublin *Medical Press* (April 20, 1853), in which syncope having supervened, M. Nelaton restored suspended animation by inverting the body, after which the face recovered its colour, and the cases terminated favourably. And in a case of M. de Vallet, in the Hôtel Dieu d'Orleans, the patient became suddenly pale, respiration ceased, and tracheotomy, artificial respiration, and galvanism were of no avail to prevent the fatal result. I conceive, then, that it is clearly established—1. That a continuance of the use of anæsthetics would in all cases, if sufficiently long employed, paralyze all the muscles supplied by the cerebro-spinal axis, including the muscles of respiration; and, secondarily, those dependent on the sympathetic system; that in all cases an overdose of chloroform may be expected to produce asphyxia, which, however, may, in most cases at least, be arrested, if observed before the cessation of the heart's action, by the methods usually employed. 2. That in some cases, altogether independently of organic disease, the heart's action may suddenly cease, even without an undue quantity of the drug having been employed; when, if the heart can be stimulated to contract, respiration (which cannot in the living body go on independently of the heart's action) may be restored and life preserved. The treatment here would be that usually adopted in syncope; for if the heart can be stimulated to contract, the respiratory muscles are capable of performing their functions; the patient is simply in the condition of one who has fainted. This class of cases must be rare, because anæsthetics (as before remarked) act on the cerebro-spinal much more readily than on the sympathetic system. 3. That in another class of cases death is produced by the simultaneous failure of the respiratory and cardiac movements; that these are the most dangerous of all; and that the treatment employed ought to combine the methods used in the first two cases.

"At one time I thought that death always resulted from asphyxia; but the reported cases before alluded to, and the observations both of myself and others, convince me that we must be prepared to meet with other and even more formidable dangers. However much this is to be regretted, I think it must now be admitted, and ought to be generally known; for if the profession and the public are to have the full benefit of anæsthetic agents, it is necessary to look the dangers and difficulties attending them fairly in the face, when, by a proper discrimination of the causes likely to lead to a fatal result, we may be

prepared *promptly* to apply those remedies more particularly suited to the case in question. We must be ready to lay down preconceived views and prejudices; and no man need be ashamed to change his opinion on a subject still new to the profession, and regarding which every day is accumulating experience and elucidating new facts.

"But to return to my case, from which I have wandered further than I intended: it is quite clear that by none of these methods was death produced. The administration of chloroform presented no peculiarities; he recovered from its effects in the usual way. When I saw him ten hours after, there was nothing about him to induce the slightest apprehension. Are we therefore to exonerate the chloroform from any share in the fatal result? Is the fact of a perfect recovery from the immediate effects of the anæsthetic agent to be received as proof that the system had entirely thrown it off, and that the condition of the individual is precisely what it was before its administration? The symptoms which on the 2d September manifested themselves, bore too strong a resemblance to those artificially induced on the 1st, not to suggest the idea that the two events were in some way connected. There was the loss of the intellectual powers—diminished sensibility, the strongest sinapisms produced no appearance of uneasiness—the respiration was impeded and stertorous—in a word, the condition in which he was on the 2d, seemed only an exaggeration of that produced on the 1st. Indeed chloroform acts by suspending the powers of the cerebro-spinal axis; its influence being directed especially to the sensitive nerves. Could we find an agent whose actions are limited to this, we would have reached the acme of anæsthetic discovery. In addition to the fatal results of asphyxia and syncope, are we to add to the dangers of administering chloroform those which may arise from a return of coma? Is it possible that the comatose state induced by chloroform may outlast the period of its administration, or even return after some hours of apparent health? This question is one which has often forced itself on my mind, and to which the mere fact of no such occurrence having ever taken place seemed hardly a sufficient answer. Convulsions or epileptic fits seem not uncommon results. Dr. Dunsmure, in speaking of his fatal case, remarks, that 'he seemed to take a slight convulsion like an *epileptic fit*, and *such as I have seen on several occasions* in people who have led an intemperate life.' Dr. Murphy, in the *Association Journal*, September 2, 1853, details two cases in which chloroform was administered during parturition; in the first of which 'distinct dyspnœa, excessive lividity of the face, and all the signs of extensive engorgement of the lungs and head' came on an hour and a half after labour; and though temporarily relieved, half an hour after 'she suddenly awoke with a return of the distressing dyspnœa, that was soon followed by *convulsions* and almost immediate death.' In the second case, two hours after the termination of labour, the patient was seized with slight cough and some difficulty of breathing: in the evening, dyspnœa had so much increased, that she was cupped to 10 ounces. She slept for two hours, 'but when she awoke the dyspnœa returned with still greater distress, and continued to increase during the night. On the following morning she became asphyxiated, and died about 2 o'clock P. M.,' 24 hours after the termination of labour. In remarking on these cases, Dr. Murphy asks the very appropriate question: Is it probable that, when no dangerous symptoms presented themselves during the administration of chloroform, fatal effects could result from it 'at a time when the vapour was dissipated, or, if any remained, its force greatly weakened, and less likely to cause such effects?' We know that the chloroform acts on the nervous centres, and that its action is usually evanescent; but is it necessarily so? After the cause has been removed, may not the effect remain? The second of Dr. Murphy's cases I have sometimes thought to have a shade of resemblance to the one I have just detailed, as far as symptoms go. The account is too scanty to enable us to form a decided opinion on this subject. Dr. Murphy states that she died asphyxiated. Now my patient died asphyxiated, *i. e.* the heart beat rapidly and convulsively after the respiration had become almost impeded; but I do not consider asphyxia to have been the *cause* of death, because, though its immediate precursor, it came on merely in the train of events. Death commenced from the brain (coma), and extended

to the nerves of respiration (asphyxia). Had there not been *post-mortem* appearances clearly pointing to different causes of death, one might have thought that in both cases the fatal supervention of paralysis of the nervous centres might have been prepared by its first artificial induction. Dr. Murphy states that it is improbable this should occur, seeing that he cannot 'find the report of a single case in which the patient who inhaled chloroform without an unfavourable symptom was attacked two hours after with dyspnœa, still later became asphyxiated and died.' And again he states that 'he cannot find a case of the kind in the whole range of surgical practice.'—'The danger—the only danger—which the surgeon dreads, is sudden death during the operation.' I do not mean to say, that in Dr. Murphy's cases death was caused by chloroform; but I do think that we have a right, derived from *à priori* reasoning, to fear that all danger is not past when the patient emerges unharmed from the anæsthetic condition; and that it is only more extended observation which can prove whether those fears are wholly chimerical. My own impression is, that in Mr. T.'s case death was not caused by the administration of chloroform; but I ground that opinion, not on the fact that no other case of the sort has been recorded, nor on the supposition that, in the nature of things, such an event might not occur, but on the appearances presented by the *post-mortem* examination, coupled with the symptoms for which he first sought my advice.

"A comatose condition was induced by chloroform, which was as evanescent as that state so induced commonly is—twenty-four hours afterward true coma supervened, and proved fatal. But the *post-mortem* examination of the head showed that this latter condition was connected with effusion of bloody serum—a common cause of coma, but not (as far as we know) ever observed as a result of chloroform. In cases where death has resulted from the employment of this agent, effusion has never been found, and in many it is distinctly mentioned that the brain was not even congested; so that the comatose state is produced without the intervention of vascular excitement, and therefore by a process different from that which was found to have occurred in the case I have related. I admit, however, that it is possible that an influence may be exerted on the brain, capable of superinducing, after a longer or shorter period, effusion of serum or other secondary effects; as we know that a person may recover from concussion, and yet sink under other results of the same cause which produced the concussion. Nor will this appear a far-fetched comparison, if we suppose, as I think we must do, that chloroform acts directly on the nervous centres. But there is a feature of the *post-mortem* examination which I now would call attention to, and that is the pus found between the parietal and visceral layers of the arachnoid. This is a rare phenomenon, and therefore not so well understood as other lesions of the brain and its membranes, which have been more frequently observed. It was found as a thin layer spread over the summits of both lobes, and adherent to the visceral portion. It did not extend to the basilar portions of the brain, and therefore can hardly be supposed to have been caused by any extension of inflammation from the mucous membrane of the nose. Indeed, neither during life nor after death was there anything to indicate that inflammation of the Schneiderian membrane had taken place. It could not have caused compression, and therefore is of importance only as proving the existence of previous arachnoid inflammation. But there was no redness or visible sign of inflammation of the arachnoid. There was much congestion of the cerebral vessels, but the products of their inflammation would have been thrown out in the subarachnoid tissues. Now I cannot help thinking that had there been so intense inflammation of the arachnoid as to have caused purulent effusion in the space of time between my visit at ten o'clock P. M. and his death twenty-seven hours after, there would necessarily have been some *post-mortem* evidence. I conceive, therefore, that the condition which led to the formation of pus was pre-existent to the administration of chloroform. The feeling of stupidity, difficulty of articulation, loss of sense of smell, &c., may all have resulted, not from the closure of the nares by polypi, but from this internal disease. There was no heat of head—no apparent increased vascular action of any sort—nothing to call attention to any other cause than the mechanical obstruction. But after calmly weighing the whole

facts of the case, I firmly believe that a low obscure form of arachnoid inflammation produced those symptoms which seemed fairly and naturally to be referred to the presence of polypi. What effect the administration of chloroform may have had in such circumstances it is difficult to say, but I feel convinced that it at all events did not set up the action which terminated so fatally.

"The *post-mortem* appearances in this case were very peculiar. The position of the pus on the free surface of the arachnoid must be held conclusive as to inflammation having existed in that membrane. Dr. Watson mentions the fact of such effusion as being rare (vol. i. p. 380), and quotes from Dr. Abercrombie a case in which it was observed in a child who had been three weeks ill; and says, that 'if simple arachnitis of an acute kind ever happen, it has not been my fortune to see or to recognize it.' While pus was found in the summit of the brain, the effusion of serum at the base was into the arachnoid cavity, and not in the subarachnoid space; and though bloody (*i. e.* red-coloured) serum was found in the ventricles in more than usual quantity, it was not excessive, nor by itself at all sufficient to cause death. There was also congestion of the bloodvessels of the brain; not inflammation of its substance. But that and the effusion of serum might, and probably did, take place within the last twenty-four hours of life—the purulent deposit I think hardly could.

"On these grounds I maintain that in the case I have related death did not result from chloroform. At the same time, I give it as my decided opinion, that until we find an agent which is capable of producing anæsthesia—inaction of the nerves of sensation—without implicating the functions of the cerebrum and motor nerves, we must be prepared to encounter occasional dangerous cases of asphyxia and syncope, and fatal ones in which these states occur simultaneously; and further, I would be inclined to expect, that cases might yet occur to prove that with the cessation of the immediate effect of the anæsthetic agent danger has not entirely ceased. In the meantime, I think it the duty of every surgeon to communicate to the profession any fact occurring in his practice in relation to this subject which presents novel and interesting features. It is in this way only that we shall learn the true merits of the case, and decide the important question of the compatibility of the anæsthetic agents at present known with all constitutions free from actual organic disease."

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45. *Detection of Blood-stains on a Knife covered with Rust.*—M. Daubrawa was requested to ascertain the existence of blood-stains on a knife which was suspected to have been used in the commission of a murder. The knife having lain a long time in a damp place, was rusted; but certain bright rust-free spots could be distinguished amid the rust. On heating the point of the blade, these spots scaled off, while the rust remained adherent; on the other hand, on immersing the knife in dilute hydrochloric acid, the bright spots remained untouched, although the rust was readily dissolved. It was probable that these bright spots were blood-stains; but as some non-nitrogenous organic acids will produce similar marks, some of the detached scales were heated in a test-tube, and, by the disengagement of ammonia from the hematine of the blood, caused a blue colour on reddened litmus-paper. The whole blade was then macerated for a long time in distilled water, which acquired a reddish discoloration, and by the aid of a lens, fibrin could be seen adhering to the blade in the situation of the bright spots. Ammonia added to the solution caused no precipitate; nitric acid gave a white precipitate; it became turbid from heat; solution of chlorine at first produced a green tint, this colour then disappeared, and white flocculi were deposited. These different fluids having been evaporated to dryness and burnt, and the residue dissolved in hydrochloric acid, demonstrated the presence of iron by its appropriate reagents.—*Journal de Chimie Médicale*, December.

AMERICAN INTELLIGENCE.

ORIGINAL COMMUNICATIONS.

Puncture of the Bladder through the Symphysis Pubis.—In our number for April last, we published an account by Dr. D. Leasure, of Newcastle, Pa., of a case of retention of urine, in which he had punctured the bladder through the symphysis pubis. We have since had communicated to us, through Dr. H. J. Bowditch, a letter from Dr. J. M. BRANDER, dated Jersey, June 10, 1854, in which he sets forth his claims as the originator of this operation. As we have already published (see preceding number of this Journal, p. 256) abstracts of a paper relative to this operation, read by Dr. Brander at a meeting of the Medical Society of London, in April of this year; and as Dr. Leasure does not claim any originality in this operation, but distinctly alludes to its having been done by others, it will be sufficient to insert the following extract from Dr. Brander's letter:—

"In support of my claim to originality in this novel operation, I would recapitulate in a few words that its practicability and superiority occurred to me during my dissections in Paris, in the year 1825, at which period I had the honour of reading an essay on this subject before a meeting of the Society of L'Athenée de Médecine, in whose published transactions it is mentioned. The paper was subsequently read in a translated and amended form before the Royal Medical and Physical Society of Edinburgh, in 1839, repeating the same at a meeting of the Medical and Physical Society of Calcutta, in the *Transactions* of which, in the year 1842, it is published (*vide* vol. viii. Part II.), with a recently practiced successful case appended.

"The operation was further brought to notice in the *Lectures on Surgery*, by Professor Sir George Ballingal and the late Sir Charles Bell, at the University of Edinburgh.

"I may mention, also, that the operation has been successfully performed on several occasions by myself and others in India, precisely after the manner adopted."

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Case of Narcotism from Opium—electricity employed in the treatment. By W. R. BULLOCK, M. D., of Wilmington, Del.—Aug. 14, 1854. Was at 12 P. M. requested to visit J. P., who was "supposed to have taken something." The patient is a man of about 40 years of age, married, and the father of several children; constitution naturally robust; shoemaker by trade; intelligence greater than is usual in persons of his position in life; addicted to the use of alcoholic liquors, though not to the extent of beastly intoxication. When under their influence he is foolish, irritable, and imaginative of evil against him on the part of others. In this state of mind, he took a quantity of laudanum about a year since, which produced nausea, and was in part rejected before I saw him. Emetics were administered, and the stomach emptied; no narcotism followed.

During the evening of the 14th, he had maintained his habitual manner, and retired about 10 P. M. About 11 P. M., the attention of his wife was attracted by the unusual character of his breathing, which, with the change

in his appearance, and the impossibility to arouse him, induced the request for medical aid at the time stated.

Found him lying on his back on a truckle-bed; countenance pallid, and bathed in a cold perspiration; lips somewhat livid; eyeballs turned up beneath the lids, and fixed; pupils contracted and immovable; head hot; insensibility to external impressions profound; respiration three to four per minute; jerking, and somewhat stertorous; extremities warm; pulse accelerated, full, and hard.

His wife stated that of late he had been drinking, but only so as to render him foolish, peevish, and suspicious; nothing having occurred to make her at the time fearful of any design on his part to repeat the attempt of last year, she found it difficult to believe that he had swallowed any poisonous drug. The breath afforded no indications whatever; deglutition impossible; venesection to twelve or fifteen ounces, when the pulse yielded, and the flow was stopped. Sinapisms to the lower extremities; cold to the head; simple enema; the greater part of the enema was retained, but no evacuation produced.

After waiting a short time to observe the effect of the treatment, and still uncertain as to his being under the influence of a narcotic, requested a search to be made of his clothing. A letter was found in the pocket declaring his intention to destroy himself, and assigning some trivial reasons therefor. A few moments after, a bottle was found near the bed, which had undoubtedly contained laudanum. No doubt now remaining respecting the cause of his condition, returned to my house (near by) for a stomach-tube, but found it impossible, from the stiffness of the tube near the point—the tube was a new one—to direct it downward from the fauces into the œsophagus, the extremity striking obstinately against the back of the pharynx, and rendering the danger of perforation imminent in case of the application of more force than was actually employed. The difficulty persisted in spite of every attention to the proper position of the head of the patient, and endeavours with the finger in the fauces to give the instrument the required direction. Then commenced the use of the magneto-electric machine (one of Kinné's), passing the current from the spine through the muscles of the chest, the abdomen, and the region of the diaphragm. At this time received the counsel and assistance of Dr. Askew in consultation. The Doctor made another attempt to pass the œsophagus tube, but encountered the same difficulty, and the same insuccess. Ice was procured, and ice-cold water kept applied by compresses to the head. A sinapism was placed along the spine, and others to the extremities, and to the region of the diaphragm—the electricity being continuously applied. Whilst under this treatment the number of respirations increased to about five per minute, the pulse remaining good. About 4½ A. M. the patient opened his eyes, roused up, spoke, and recognized those about him; appearing also to be conscious of his situation. He sighed frequently and deeply; and the respiration became gradually more regular. Ammonia, and cold water, were now administered. There was some difficulty in deglutition, and a disposition to hawk and spit. The electricity was dispensed with, and some of the sinapisms removed.

He remained conscious and rational for about three-quarters of an hour, then complained of great weakness; broke out into a profuse perspiration, and rapidly sank into a condition even more unpromising than before. The electricity and sinapisms were immediately resumed. The pulse, which, during the state of consciousness, had risen to be so full and hard as to make me think that the lancet might be again required, diminished greatly in force,

whilst its frequency increased to from 150 to 160 per minute. The number of respirations was reduced to two or three per minute, and that under the influence of the full power of the machine passed from the neck in the course of the phrenic nerve to the region of the diaphragm. The current was also passed through the region of the medulla oblongata, and through the entire course of the spine, the muscles of the chest and abdomen being in like manner alternately stimulated. Frictions were also made to the chest and extremities. The electric current was particularly effective in the production of the inspiratory act when passed in the direction of the course of the phrenic nerve. The ribs would at all times be somewhat elevated when thus applied; but the sudden, sighing inspiration could be produced only at regular intervals, which were generally waited for. The inspirations thus excited were commonly followed immediately by one other more feeble in power. These efforts were steadily continued until 8 A. M., at which time the patient was again seen by Dr. Askew. The respiration had not at all increased in frequency, whilst the force of the pulse had declined; the lividity of the surface, and the prostration being so great at this juncture, that a much longer continuance of life appeared almost hopeless. About 9 A. M. the pulse had perhaps gained a little in force, and the warmth, which had diminished in the extremities and had left the head, was gradually restored; so that cold cloths were again applied to the latter. In this condition, with slight fluctuations in the heat of the surface and force of the pulse, the patient remained throughout the morning; the sustained action of the heart, which, throughout, seemed disproportionate to the respiratory power, affording the only plausible ground for hope that, by maintaining the respiratory act, a favourable issue might possibly ensue.

About 1 P. M. put the machine into the hands of attendants, with directions to keep up the action, and left to attend to some affairs of urgence. Returning about 2 P. M. found a person called to state that from some cause or other the machine, in their hands, had ceased to operate a short time after my departure, but that about half-past one P. M., to the astonishment of all present, the patient again roused up. Saw him immediately, and found him rational when his attention was strongly called; eyes open, and nearly natural in aspect; mind wandering when not spoken to, and disposed to revert into stupor. The respiration, though still very slow, was much improved; sighing, and the force of the pulse, was increased; head so hot that ice-cold water was steadily applied; much thirst. Cold water was frequently given in moderate quantities; also ammonia, strong coffee, and beef-tea. Determined, if possible, to prevent a relapse into stupor, to which there was a strong disposition. The sinapisms were continued, as also momentary applications of an electric current of diminished power. The extremities were rubbed, and warmth applied to the feet. By these means the patient was kept awake until evening, by which time the tendency to drowsiness was almost dispelled, and he sat up to take his drinks. Visited him at 8 P. M., leaving directions to allow him to sleep, but to be carefully watched, and roused if symptoms of prostration or stupor should occur; also to give a little ammonia and nourishment during the night.

Aug. 17. Passed a comfortable night, though somewhat disturbed by dreams and phantoms; better in every respect; complains of great weakness; pulse good, force moderate; bowels not opened; tongue much coated, and brown and dry; some nausea, with an occasional rejection of an acid watery fluid. From this time nothing of particular interest presented; the tongue cleaned after the administration of mercurial cathartics, which, how-

ever, operated with difficulty; and the patient was in a couple of days about his business, as usual. On inquiry, it was learned that the quantity of laudanum swallowed amounted to from f℥ij to f℥iiss; not a drop of which was removed from the stomach. The electricity was continuously applied for nearly twelve hours up to the time of the last return of consciousness.

—

Case of Rupture of the Uterus at the Moment of Delivery. By HUTTERSLY P. WORTHINGTON, M. D., of Maryland.—Dec. 16, 1851, I received a hurried summons to Mrs. S., a strong, robust, hard-working woman, the mother of eight children. Upon my arrival, I found the child, a healthy female one, delivered about fifteen minutes, but the mother much prostrated from profuse hemorrhage. Her attendant, an intelligent and experienced female, informed me that the labour had been rapid, but as the child's head passed the vulva, there was a copious dash of blood, which continued until and after the delivery. The placenta was still retained. My attention was not immediately attracted to the full amount of the hemorrhage, being rather directed to ascertain and remove the cause. Administering a copious draught of brandy, and applying cold cloths to the abdomen, following the course of the cord, I introduced my hand to remove the placenta; finding the uterus contracting, and not immediately reaching the placenta, I pursued my exploration for it, when I suddenly felt my hand slip through a jagged strictured orifice, and in contact with what I at once recognized as a portion of the intestine. The uterus was ruptured at its fundus; the placenta had slipped into the peritoneal cavity, and could not be reached. I continued the use of stimulants, with the effect of stopping all further hemorrhage externally; but a sudden rigor, twenty minutes after my arrival, showed that further efforts would be unavailing; the patient soon expired. I regretted my inability to obtain consent for a *post-mortem* examination.

This woman had for two or three months previous, though apparently in good health, expressed and acted upon the conviction, that she would not survive this labour. I was struck with her frequent enunciation of this conviction; and, as she was of an equable and cheerful disposition, and not likely to have expressed such fears without some morbific impression originating the feeling, I had endeavoured to discover any reason for it, but without avail. I had attended her in several former labours, all of which were easy. What connection could there have been between the cause of her death and this state of feeling? In our ordinary fevers, more particularly those of a congestive type, we frequently meet with instances of this prescience of death before active febrile symptoms have developed themselves, and account for them as effects of the morbific poison in its primary depressing action upon the system. Could there have existed a softened or diseased spot, say of ulcerative inflammation, in the *fundus uteri*, which during gestation, without giving rise to much general disturbance, could yet, through nervous depression, account for the woman's prescience, and which caused so weakened a state of the parietes as to cause them to give way during labour? A *post-mortem* examination might have elucidated this question.

A truly remarkable feature in this case, was the point of time at which the rupture took place; not until the head was low in the pelvis, and when there was no obstruction to its progress.

LAUREL FACTORY, *Maryland.*

DOMESTIC SUMMARY.

Case of Total Inversion of the Uterus, in which Extirpation of the entire Organ was successfully practised.—Dr. E. GEDDINGS, Professor of Surgery, &c. in the Medical College of the State of South Carolina, relates (*Charleston Med. Journ.* Sept. 1854) the following case:—

"On the 16th of May, 1854, I was requested by Dr. A. P. Pelzer, to meet him in consultation, in the case of a negro woman belonging to Mr. Robt. White, in King Street. On my arrival, Dr. Pelzer called my attention to a large pyriform tumour, equal in magnitude to a fœtal head at the full term, which, proceeding from within the vagina, hung pendant between the thighs. This tumour was large and rounded below, but contracted into a rather thick pedicle above, which could be traced about three-fourths of an inch within the vulva, at which point its contour was surrounded by a kind of *cul-de-sac*, beyond which the finger could not be passed. Its whole surface was covered by a rough, thickened mucous membrane, abraded and ulcerated on many points, considerably inflamed, and disposed to bleed when roughly handled. In the general aspect, it bore a strong resemblance to a case of prolapsus of the uterus, of long standing, but the uniform roundness of the most dependent part, together with the absence of the os tincæ, served at once to convince us that it was of a totally different nature.

"The first supposition that presented itself to my mind was, that it might be a case of prolapsus of the bladder, of such long duration, that the walls of the organ had become very much thickened, and otherwise altered in texture. But on introducing the catheter, and passing my index finger around the neck of the tumour within the vulva, I was enabled readily to discover that it was a case of complete inversion, with extensive hypertrophy of the uterus, of ancient date. The orifice of the urethra was but little removed from its normal position, and in passing my finger up, on the posterior and lateral aspect of the neck of the tumour, as far as the reflected walls of the vagina would allow it to reach, I could distinctly discover the elastic feel imparted by the convolutions of the small intestines, which rested on the partially inverted walls of the vagina.

"How long the inversion had existed, could not be satisfactorily ascertained; but as there is reason to suspect that the accident must have occurred at the period of her last delivery, an approximative conclusion may be drawn from the fact, that her youngest child, a daughter, was present, and had the appearance of a person of from eighteen to twenty years of age. The report of the woman herself was, that she had been greatly annoyed by the tumour for many years, but had generally been enabled, by partially forcing it up into the vagina, and sustaining it there by means of a T bandage, to pursue her ordinary avocations. Latterly, it had increased so much in size as to render this impracticable, and at the period of our visit, any attempt at replacement, however partial, was productive of excruciating pain. She was, besides, suffering so much from engorgement and inflammation of the inverted organ, that, considering this, together with the partial and uncertain benefit likely to accrue from any merely palliative treatment, it became a serious question how we could most readily and efficiently relieve our patient.

"Reflecting on all the circumstances of the case, it occurred to me that excision of the entire inverted organ presented a rational prospect of relieving not only the present sufferings, but also the cause of much future annoyance. The vagina being also partly inverted, the danger of such an operation was materially diminished, inasmuch as we would in consequence of that condition, be enabled to excise the entire mass by cutting through the vaginal walls, thus leaving the substance of the uterus untouched.

"Dr. Pelzer concurring with me, I seized the neck of the tumour as high up as possible, between the thumb and index finger, and manipulating in such manner as to satisfy myself that it contained none of the convolutions of the intestines, I proceeded to include it in a strong ligature, for the twofold purpose of preventing the protrusion of the intestines, and obviating any serious hemorrhage.

The neck of the tumour was then cut through, a little below the ligature, with a single swipe of a probe-pointed bistoury.

"The operation was exceedingly simple and easy; was attended with no great pain; and, as may be supposed, was executed in a few seconds.

"The after-treatment presented no features of particular interest, and the case progressed so favourably that after a few days, I was enabled to discontinue my visits, leaving the patient in the hands of Dr. Pelzer, who in a short time, transferred her to Prof. Frost, the family physician, who, at the period of our attendance, was absent from the city. She speedily recovered, and, as I understand, has since done well.

"On making a section of the tumour, it was found to present a solid homogeneous mass, of a grayish-white texture, and fibrous appearance. The whole cavity formed by the inversion of the walls had become obliterated by adhesions between the opposing peritoneal surfaces; but the point of junction between the vagina and the contour of the cervix could be distinctly recognized, the incision, as stated above, having passed through the walls of the vagina.

"Partial and complete extirpation of the uterus, for various objects—inversion, prolapsus, carcinomatus and other degenerations of its structure—has been so often practised, that the simple operation and the description of which I have detailed, possess no claims to interest in point of novelty; yet it has some value as affording an additional instance to prove that, under similar circumstances, the unfortunate victims of a displacement so deplorable, may often be relieved of much suffering and inconvenience. It might be interesting to collect full references to the numerous cases in which extirpation has been practised on account of inversion, but as I have not time to execute the task, I must content myself with this brief and imperfect exposition of a single case.

—

Treatment of Rheumatism.—Dr. Fuller, in his recent work upon rheumatism, regards the pathology of the disease as depending upon the presence of a "*materies morbi*," which he supposes to be an excess of lactic acid, owing to defective assimilation, or suppressed perspiration. With the view to the neutralization and elimination of this acid, he recommends the free use of alkalies, by which means he "hopes the average duration of an attack may be reduced, from a month or six weeks, to ten days or a fortnight."

Dr. John B. Chapin, states (*New York Medical Times*, Aug. 1854), that during the attendance of Dr. Swett, at the New York Hospital, the present year, all the patients admitted with acute articular rheumatism, were put under the "alkaline treatment;" and he gives a statistical table of twenty-five cases treated by this method.

The salt chosen was the tartrate of soda and potassa, a neutral salt, possessing the property of rendering the urine alkaline.

"The plan of treatment usually pursued was, if the patient presented himself with unusual excitement of the skin and pulse, to administer a mixture of sulphate of magnesia and tartarized antimony, until the skin was relaxed, and the pulse reduced to a more natural standard. The Rochelle salt was then directed, in drachm doses, every two or three hours during the daytime, till the urine was rendered alkaline, when it was gradually suspended. A lotion of carb. potass. ℥j, with opium ʒij to the pint of water, was directed as an external application. The administration of the salt was not attended with disagreeable consequences, with the exception occasionally of some ulceration about the fauces—in no case was its action so severe upon the bowels as to require its entire suspension. The persons attacked were in the full vigour of health, and the character of the disease acute in its form. The frequency of administration of the remedy was governed very much by the reaction of the urine.

"On the admission of the patient, the urine was tested, and, in all cases, was found to be of acid reaction, and the secretion of the skin presented the usual acid odour. The treatment was generally commenced the second or third day after admission, and the urine was rendered of decided alkaline reaction in an average of five days after its commencement; the longest period it resisted the alkaline reaction having been twenty days, and the shortest two. The secretions of the skin have not, I believe, been noticed to alter. In one case, attended

with profuse perspiration, which yielded readily to treatment, the coloured shirt the patient wore entirely lost its colour; and it was suggested whether the same change did not take place in the perspiration as in the urine. The average amount of the salt administered was from five to seven ounces.

"The average date of commencing improvement was seven days after commencement of treatment, coinciding, in the large majority of the cases, with the commencing alkalinity of the urine. The improvement was invariably permanent, and after the urine was rendered alkaline, *no new articulations were effected*, as a general rule.

"The average period of convalescence was twelve days after admission; and the whole duration of the disease, including the period previous to admission, was twenty-two days. Of thirty cases treated by Dr. Swett, during April and May, 1853, during which time no uniform course of treatment was pursued, the average duration was five and a half weeks. One of the most gratifying results of the alkaline treatment was the diminished frequency of cardiac complications. Twenty-one of the twenty-five were free from any complication, three were *admitted* with aortic obstruction, and one with mitral regurgitation. Not one patient was attacked with any heart complication during the treatment of the disease. Comparing this result with the practice last year, it was found that four had mitral regurgitation, six aortic complication, and three suffered from pericarditis; thirteen in all, out of thirty.

—

Hemeralopia.—Dr. C. S. Fenner, of Memphis, Tenn., says (*N. O. Med. and Surg. Journ.* Sept. 1854) that he has met with a considerable number of cases of this affection among negroes working in an extended range of prairie plantations, commencing in Pickens County, Alabama, and extending up through Noxubee, Lowndes, Oktibbeha, and Monroe Counties, Mississippi.

The disease makes its appearance almost invariably in the spring, or early in the summer months, before the young corn and cotton plants have attained sufficient size to shield the eye from the dazzling reflection of the sun's rays from innumerable small pieces of limestone, and fragments of shells that are thickly interspersed through the soil. These prairies constitute immense bodies of a peculiar black soil, lying on a strata of limestone. In many places, the rock approaches the surface, and is entirely bare, reflecting the sun so powerfully as to be almost insupportable to the eye. To a person standing in the middle of some of these large prairies, the horizon, in many directions, seems to meet the surface without a tree or shrub to obstruct the view. I attribute the frequent occurrence of night-blindness through this section of country, to the exhaustion of the nervous power of the retina from continued exposure day after day to the powerfully reflected solar rays, without having any objects, even at a distance (particularly trees covered with green foliage), to relieve the eye, by frequently looking up and bringing a change of scenery within the field of vision.

"Counter-irritation," he adds, "from a succession of blisters, and repose within doors for a few days, are usually all that is requisite to give entire relief. I have been informed of several cases that recovered after the lapse of a few months, without any treatment whatever; and in no instance have I known any permanent injury to vision result from permitting the disease to take its own course."

—

Nitric Acid as an Anti-Periodic.—Dr. Geo. Mendenhall calls attention (*Western Lancet*, Aug. 1854) to the use of nitric acid in the treatment of intermittent fever. The facts upon which his paper is based are mainly taken from an Inaugural Dissertation, by Dr. E. T. Bailey, of Emmetsville, Indiana.

The latter states that in the section of country in which he resides, there is a large portion of marshy land, and, therefore, the circumstances are favourable to the development of autumnal fevers. His attention was first attracted to the use of nitric acid in the treatment of intermittent fevers, by noticing its effects in a case of chronic intermittent, which was attended with profuse night-sweats, and for which complication he administered the remedy. In this case, there had been daily paroxysms for the preceding five days; night-sweats profuse, the tongue coated, and the bowels constipated. Nitric acid was given in doses

of six drops, diluted with water, in the evening; and he was agreeably surprised to find that the paroxysms did not return on the following day; and this circumstance induced him to try its effect in other cases as an anti-periodic.

Since that time he has treated over ninety cases of intermittent fever with this article, with remarkable success. Of this number, all recovered promptly except ten; and in every one of these unsuccessful cases, the remedy was discontinued contrary to directions.

Fifteen of the whole number were of the tertian type, and seventy-five of the quotidian. In fifty cases, there was no return of the chill after commencing the use of the acid. The others were rarely attended by more than one paroxysm, and in no case by a third. When the patient had a paroxysm after taking the medicine, it was in every case diminished in intensity and duration.

In Dr. Bailey's practice, this remedy has entirely superseded every other article for the purpose of interrupting the paroxysms of intermittents. His mode of proceeding is to give from five to eight drops of the commercial nitric acid, properly diluted, once in six hours, without regard to intermissions or exacerbations. Cathartics and alterants may be necessary for the purpose of changing certain conditions of the system; but so far as the interruption of the paroxysms is concerned, the acid may be given without any preparation of the system whatever, if we choose to do so.

Remedial Properties of Simaba Cedron.—The number of the *New York Journal of Medicine* for September contains an interesting paper by one of the editors, Dr. S. S. Purple, on the remedial properties of the Simaba cedron, and on its employment as a substitute for quinia. The following are Dr. P.'s conclusions:—

"That it possesses decided anti-periodic properties, and is, therefore, applicable in the treatment of periodic diseases.

"That it is less likely than quinia to produce the aggregate of encephalic or neuropathic phenomena, induced by overdoses.

"That it may, in large doses, repeated often, produce griping of the bowels, and even diarrhœa; but that these conditions are easily controlled by appropriate medicaments.

"That, as a remedy in intermittent fever, it possesses properties in many respects equal to quinia, and in most cases is equally adapted to the curation of this disease.

"That, in the treatment of yellow fever, it does not appear to possess any particular advantages over quinia, but, nevertheless, is equally well adapted to fulfil the indications which call for the use of this latter remedy.

"That it possesses marked tonic properties, and deserves a prominent place in this classification of the Materia Medica.

"That in chronic dysentery, diarrhœa, dyspepsia, and all states of the stomach accompanied with impaired or difficult digestion, its use will be found to be attended with benefit.

"That, should a demand arise for its use in medicine, it is believed that it will be found not difficult to obtain a supply, in quantities sufficient to afford it at a much less price than quinia."

Amputation at the Shoulder-Joint. Dr. M. Carnochan records (*American Medical Monthly*, Sept. 1854) a case of amputation at the shoulder-joint "for the removal of a large osteo-procancerous tumour of the humerus." The subject of the case was an Irishman, thirty years of age, admitted into the State Emigrant's Hospital. The tumour involved the whole extent of the humerus, measured in its largest circumference twenty-nine inches, and weighed, after removal, eighteen and a half pounds.

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